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THE PRESSURE OF LIGHT ON GASES

AN EXPERIMENTAL STUDY FOR THE THEORY OF COMETS' TAILS

By PETER LEBEDEW

The peculiar forms developed in the tails of comets in the neighborhood of perihelion led Kepler,[1] almost three hundred years ago, to the thought that the sun's rays exert pressure upon the matter vaporized in comets' heads and repel it from the sun.

Additional weight was later given to this idea by Fitzgerald,[2] when he sought to base such an effect of the rays on Maxwell's force of pressure. In order to be able to compute the magnitude of the forces occurring, Fitzgerald first proceeded on the assumption that the separate gaseous molecules are absolutely black spheres, and that these spheres behave in respect to the incident light-waves in the same manner as would black spheres of very much larger dimensions. In the case of very small spheres, the phenomena of diffraction become significant, as was proved by Schwarzschild,[3] who rigorously computed the pressure of light on small perfectly reflecting spheres. Debye[4] solved this problem in a general way for small bodies of any desired constitution, and thus it is possible to subject

[1] J. Kepler, *De Cometis.* Augustae Vindelicorum 1619 Opera Omnia. Ed., Dr. Ch. Frisch. **7**, 110, Frankfort, 1868.

[2] G. Fitzgerald, *Proc. Roy. Dublin Soc.*, **3**, 344, 1883.

[3] K. Schwarzschild, *Sitzberichte der Münchener Akademie der Wissenschaften, Math. Klass.*, **31**, 293, 1901.

[4] P. Debye, *Annalen der Physik* (4), **30**, 57, 1909.

to an accurate quantitative treatment the investigations suggested by Arrhenius,[1] which deal with the pressure of light on cosmical dust. It is not permissible for us to apply to the material of comets' tails, which, from their spectroscopic behavior, we must consider as consisting of separate fluorescing gaseous molecules, the computations which are valid for small spheres, as I pointed out long ago,[2] and in view of my proof that the separate molecules must be treated as resonators with selective absorption. Experiments which I made[3] with acoustic waves permit the observation of the continuous effect of these waves on movable acoustic resonators as sharply defined phenomena; the computations[4] which I made for electromagnetic waves permit us to infer an analogous effect of light-rays on separate molecules of gas. Debye[5] treated thoroughly the light pressure on a schematic molecule (a vibrating bipolar body) which is exposed to the solar rays in the same way as the gaseous molecules of a comet's tail; he then computed the numerical values of the repulsive forces thus arising.

Although the computations thus made, and the analogy with the acoustic resonators, scarcely leave any doubt as to the correctness of the idea proposed by Kepler, nevertheless it seemed to me to be in the interests of a theory of comets' tails based upon physical experience to see if direct experiments in the laboratory would give an unimpeachable proof of the repulsive effect of light on gases. Inasmuch as we are unable to deal with single molecules in such experiments, we are compelled to investigate the effect of light on a mass of gas which is compounded from the separate effects of the individual molecules. The resulting effect can readily be computed in this case, as was indicated by Fitzgerald,[6] who proceeded on the simple assumption that those rays only would exert Maxwell's pressure which were absorbed by the gaseous mass, and which, therefore, behave with respect to the gaseous mass like a black body. Then,

[1] S. Arrhenius, *Physikalische Zeitschrift*, **2**, 81–97, 1901. See also *Lehrbuch der kosmischen Physik*, and *Das Werden der Welten*, Leipzig, 1908.

[2] P. Lebedew, *Wied. Ann.*, **44**, 292, 1892.

[3] *Ibid.*, **62**, 168, 1897.

[4] *Op. cit.*, p. 170.

[5] *Op. cit.*, p. 97.

[6] *Op. cit.*, p. 345.

in case of a beam of parallel rays, the repulsive force p in the direction of the ray will be

$$p=\frac{aE}{V},$$

where a is the coefficient of absorption of the energy E incident per second of time, and V is the velocity of light.

I. *Method.*—If a beam of rays of white light passes through a selectively absorbing mass of gas, then the mechanical forces which are to be expected must reveal themselves by the fact that the gas thus penetrated begins to displace itself in the direction of the motion of the light. Inasmuch as the coefficients of absorption of gases are in general very small, the repulsive forces developed, even under the most favorable conditions of the experiment, hardly amount to the hundredth of the pressure which the same beam of rays would exert upon a solid black wall. In order to be able to observe these small forces, the experiment had to be so arranged that the gas could freely move in the direction of the beam and act upon a sensitive valve which could not be directly affected by the beam of rays. Fig. 1 represents the apparatus constructed for this purpose: the gas is placed in a parallelopipedal cavity G, having windows, F_1 and F_2, of fluorite, and is so traversed by the bundle of rays L_1 L_2 that no rays fall upon the walls. If the beam of rays L_1 L_2 exerts a force of translation upon the mass of gas, then there must develop at the windows, F_1, F_2, differences of pressure in the gas which may become equalized by the dark space at the sides. This space at the sides is (almost) closed by an easily movable valve P; the valve P being hung on one arm of a torsion balance, the difference of pressure thus arising can be measured by the displacement of the valve P. After we have measured the diameter of the valve P, the directive force of the quartz-fiber Q, the length of the lever-arm of the torsion balance T, and the distance of the scale from the mirror of the reading telescope, we may readily compute in absolute measure the difference of pressure which corresponds to the deviation of the pressure apparatus by one scale-division. In this apparatus one scale-division $=1.4\times10^{-6}$ dyne per sq. cm. A pencil of a Nernst lamp N (Fig. 2) served as a source of light: its rays were thrown on a rectangular diaphragm D, 2×3 mm, by the concave mirror S; then fell upon an

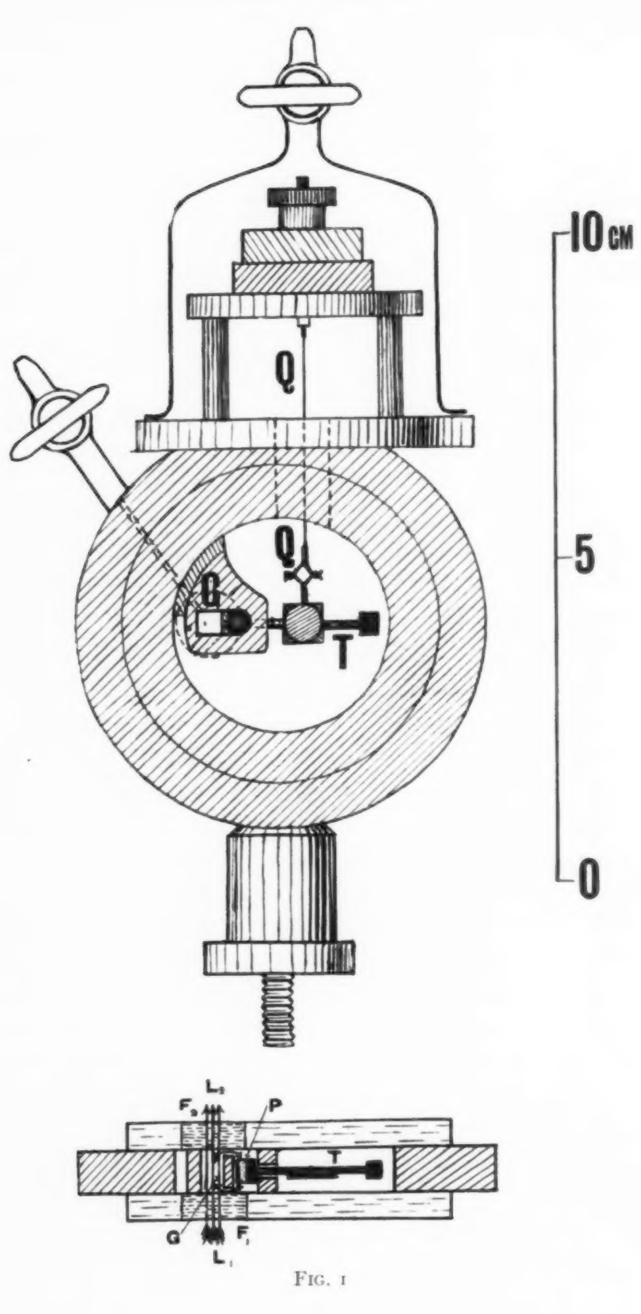

FIG. 1

inclined plane mirror P_1, and were united in a real image of the diaphragm by the concave mirror S_1 in the gaseous space G (Fig. 1) of the above-described pressure apparatus. The plane mirror P_1 can be replaced by P_2 without jar, by a pneumatic attachment, and the rays are sent by S_2 through the gaseous space of the pressure

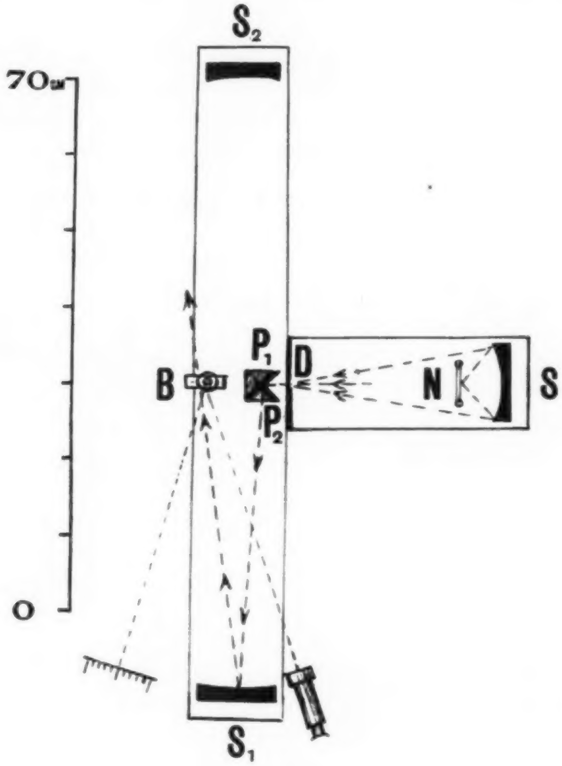

FIG. 2

apparatus, in the opposite direction. Thus the change in direction of the rays doubles the deflection of the pressure apparatus due to the pressure of light on the gas, while the direct effects, which are due to the radiation of the warmed gas on the valve of the pressure apparatus, as a result of the unavoidably small differences of symmetry in the apparatus (which are independent of the direction of the effective rays) disappear.

The coefficient of absorption *a* of the gas to be investigated was determined by the aid of two thermo-elements T_1 and T_2, which were attached close to the fluorite windows; the ratios of their electromotive forces were determined by the galvanometer when the space for gas was filled, first with air, and then with the gas under investigation, and thus the coefficient of absorption was derived in a simple manner.

The energy of the beam was measured with a calorimeter by allowing the rays to fall for five minutes upon a block of copper *K*

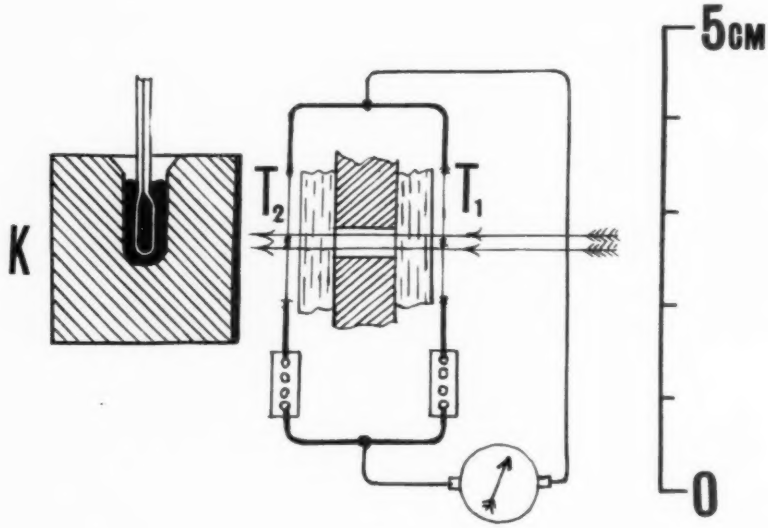

FIG. 3

(Fig. 3) previously given a black coating of platinum on its front surface, and having a known water-equivalent; the rise in its temperature was then measured. I cannot here go into the details of this very difficult experiment, but refer to its more extended description in a paper to appear in the *Annalen der Physik*. I will here only briefly mention that obstacles of two kinds hinder the quantitative experimental testing of the relation proposed by Fitzgerald:

a) The beam of rays can exert an appreciable translatory effect only upon gases which absorb selectively and which, therefore, are warmed by the radiation; change in their density gives rise to con-

vection currents, and thereby can displace the valve of the pressure apparatus. These disturbing effects of the warming can be determined, however, by exhaustive investigations, and are not injurious when the apparatus is correctly set up. As these disturbing forces are decidedly smaller in mixtures of hydrogen than in pure gases, the definitive experiments were made solely with hydrogen mixtures.

b) The simple relationships which Fitzgerald gave for a parallel beam cannot be realized experimentally, since in this case the energy of the beam cannot be made large enough. In a convergent beam the gaseous mass is not penetrated uniformly, differences of pressure arise in its interior, and the accurate computation of the effect of these differences of pressure on the valve apparatus cannot be made; we are, therefore, limited to estimates of the disturbing effects, and thus the computation of the absolute values of the forces of pressure to be measured from a and E are rendered decidedly more difficult and uncertain.

The result is that the relationships given by Fitzgerald can be tested quantitatively only to within about ± 30 per cent. It seemed to me necessary that I should content myself with this precision because the question as to the existence of the translatory effect of light on gases could be definitely decided, and because, on the other hand, the attainment of a greater precision was hindered by very great experimental difficulties.

II. *Results.*—The results of the definitive measurements are summarized in the following table, in which N denotes the current number of the observation, β the measured deflection in scale-divisions of the pressure apparatus, a the measured coefficients of absorption, and T_0 the measured rise in temperature of the calorimeter in five minutes, by which the incident energy E of the beam is measured. The column P_m contains the absolute amounts of the directly measured pressures of the light on the gas, in millionths of a dyne per sq. cm, as determined from the measured deflections β of the pressure apparatus, from its linear measurements, and from the torsion of the quartz-fiber.

In column P_c are given the pressures, also in millionths of a dyne per sq. cm, computed according to Fitzgerald from a and E. The ratio $P_m : P_c$ should be constant and differ only slightly from unity.

The table includes twenty series of observations made with four different Nernst pencils as sources of light. This explains the different values of intensity of the ratio T_0 and of the absorption coefficients a for the same mixtures of gases.

N		β	a	T_0	P_m	P_c	$P_m : P$
3......	0.5 Methane + 0.5 H_2	0.65	0.0065	0.48	0.91	0.76	1.20
6......	" " " "	0.60	0.0057	0.46	0.84	0.66	1.27
20......	" " " "	0.70	0.0071	0.55	0.98	0.98	1.00
1......	0.5 Propane + 0.5 H_2	2.05	0.0200	0.42	2.86	2.10	1.36
2......	" " " "	1.75	0.0175	0.43	2.45	1.89	1.30
11......	0.5 Butane + 0.5 H_2	2.10	0.0179	0.48	2.95	2.15	1.37
12......	" " " "	2.00	0.0172	0.48	2.80	2.06	1.35
13......	" " " "	3.10	0.0189	0.64	4.34	3.03	1.42
15......	0.1 Butane + 0.9 H_2	0.55	0.0063	0.55	0.77	0.87	0.88
17......	" " " "	0.70	0.0072	0.54	0.98	0.97	1.01
19......	" " " "	0.65	0.0067	0.55	0.91	0.93	0.98
4......	0.5 Aethylene + 0.5 H_2	0.60	0.0068	0.43	0.84	0.73	1.14
9......	" " " "	0.75	0.0075	0.50	1.05	0.94	1.12
16......	" " " "	0.80	0.0075	0.55	1.12	1.04	1.08
5......	0.5 Acetylene + 0.5 H_2	0.85	0.0080	0.50	1.19	1.00	1.19
10......	" " " "	0.85	0.0068	0.49	1.19	0.83	1.43
18......	" " " "	0.70	0.0063	0.53	0.98	0.77	1.27
7......	0.5 Carbonic Acid + 0.5 H_2	0.55	0.0055	0.50	0.77	0.69	1.11
8......	" " " " "	0.55	0.0061	0.48	0.77	0.73	1.05
14......	" " " " "	0.70	0.0072	0.51	0.98	0.92	1.06

The table shows that for each mixture of gases, the series of observations agree on the average within 10 per cent, corresponding to the possible errors of observation of the separate measures. For different mixtures of gases, in which the coefficients of absorption vary as 1:3 (methane and butane), and the density as 1:4 (butane), the ratios $P_m : P_c$ exhibit differences which lie outside the errors of observation and indicate slight instrumental errors in the adjustment which could scarcely be overcome in such exceptionally difficult experiments.

The results obtained may be summarized in the following manner:

1. The existence of the translatory force exerted by light upon gases is experimentally established.

2. These forces are directly proportional to the quantity of energy incident and to the absorption coefficients of the masses of gas.

3. The relationship proposed by Fitzgerald is to be regarded as quantitatively proved within the limit of errors possible in these experiments and computations.

These experiments refer to masses of gas under atmospheric pressure, and the numerical values found cannot be directly applied to the excessively rare gases of comets' tails. They give, however, an experimental basis for the further exhaustive development of the physical theories of comets' tails first propounded by Kepler.

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THE INTRINSIC BRIGHTNESS OF THE STARLIT SKY

By CHARLES FABRY

Important researches have been made in recent years on the number and distribution in the sky of stars of different magnitudes. To these studies are allied some of the most important problems of cosmogony and physics, such as the absorption of light and the distribution of stars in space. The statistical data which serve as a basis for all these calculations are so difficult to obtain that it is not without interest to find a direct verification of them. Such a verification may be sought by the measurement of the total light given by the starlit sky, or, better, of the mean intrinsic brightness of the sky in different regions of the celestial sphere. The importance of such determinations has been indicated by Newcomb, who concluded that these numerical data "must be considered among the most important fundamental constants of astrophysics."

Two attempts, only, to measure have been made thus far—both visually and by elementary means, by Newcomb,[2] and by Burns.[3] The results obtained by these two observers can be summarized as follows:

1. From one region to another of the sky, no very large differences in the intrinsic brightness are found. Newcomb does not find any difference between the regions whose galactic latitude is greater than 25°; in the most brilliant parts of the Milky Way the brightness would be two or three times that of the non-galactic sky. This last result is also given by Burns.

This slight increase of intrinsic brightness in the Milky Way is, as Newcomb remarks, altogether unexpected. From the statistical data on the distribution of stars, Newcomb should expect to find in the Milky Way a brightness ten times greater than that at the galactic pole, and at 30° galactic latitude a brightness twice that at the pole. Instead of the numbers 10 and 2, the measures give 2 and 1.

[1] *Astrophysical Journal*, **14**, 297, 1901.

[2] "A Rude Attempt to Determine the Total Light of All the Stars," *ibid.*, **14**, 297, 1901.

[3] "The Total Light of All the Stars," *ibid.*, **16**, 166, 1903.

2. A square degree of non-galactic sky would be equivalent, according to Newcomb, to 1.15 stars of the fifth magnitude; according to Burns, to 2 stars of the fifth magnitude. These two numerical results are not as concordant as one might wish; it is necessary to remark, however, that Newcomb evaluates the probable error of his result at 25:100, and that that of Burns is the mean of numbers which vary in the ratio of 1:2.

The authors of statistical studies do not appear to have given much consideration to these measures of the intrinsic brightness of the sky.

Method.—Visual measures are difficult on account of the faintness of the intrinsic brightness to be measured. Photographic measures, on the contrary, are very easy, without excessive exposure-times, because each point of the plate can receive a very wide cone of rays, whereas the retina cannot.

The very simple apparatus which I have used is represented in Fig. 1. The objective A (which will be called the telescope objective)

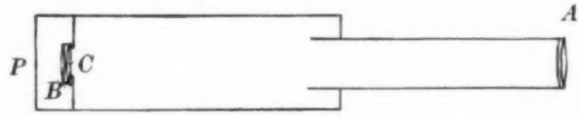

FIG. 1

has a focal length $F=48$ cm, and an aperture $D=5$ cm. In its focal plane is a diaphragm C, pierced by a circular aperture the diameter of which will be called d. Immediately behind this aperture is an optical system B, of very short focus and large aperture, having, consequently, qualities analogous to those of an objective of a microscope, and which will therefore be called the microscope objective. It is not necessary that this objective should have good optical qualities; it consists, in my apparatus, of two simple lenses, each of 20 dioptries, forming a system of focal distance $f=3.15$ cm, and of 3.5 cm usable diameter. This optical system projects on the photographic plate P the image of the telescope objective A. The whole forms a sort of photographic telescope, mounted on a simple, jointed support, supplied with leveling screws. A coudé finder, attached to the apparatus, makes it possible to point it exactly toward any desired region of the sky and, in particular, to center the image of a given star exactly on the opening of the diaphragm C.

Whatever the aperture of the diaphragm, and whatever the distribution of the stars in the region observed, there is formed on the photographic plate an image, uniformly illuminated, of the telescope objective A, the image being a circle 3 mm in diameter. It is formed by the light of all that portion of the sky of which the image is in the aperture C. The measure of the intrinsic brightness of a region of the sky will include two successive exposures, one on a comparison star, and one on the region of the sky chosen:

1. Let the aperture C be of very small diameter, and center the image of the comparison star on that aperture. The image obtained is then produced solely by the light of the star.

2. Next direct the telescope toward the region being studied, and give to the diaphragm a large aperture. The image is then formed by the light of all that portion of the sky which is projected through the aperture, the angular extent of which can easily be calculated. After a few trials, the diaphragm can be opened so that the photographic impression will be the same as in the first instance, the time of exposure being the same. An extremely simple calculation then gives the luminous intensity of one square degree relative to that of the comparison star.

For comparison star I have chosen *Polaris*, as being convenient on account of its almost complete immobility in the sky, and as having an intensity suitable for easy measures with my apparatus. In measures on the non-galactic sky, I was led to use a diaphragm of diameter $d=3$ cm; with Lumiere "Sigma" plates this gave a satisfactory impression in 10 minutes.

Instead of varying only the diameter of the diaphragm, it is more convenient, and not less precise, to proceed in the following manner: By a preliminary trial, determine once for all the diameter which it is necessary to give to the aperture of the diaphragm so that the exposure on *Polaris* and that on the region of the sky chosen should give, in the same time, about the same impression. The measures relative to this region of the sky will then be made with the aperture thus found but with varying times of the exposure. For example, having found that an aperture of 3 cm is satisfactory, make an exposure of 10 minutes on the region being studied with the diaphragm of 3 cm; then make exposures of 5, 10, and 15 minutes on *Polaris* (with a very

small aperture, so as to exclude all light except that of the star). On the developed plate, measure the opacity of the different exposures, drawing, for the exposures made on *Polaris*, the curve of opacity (or better, of its logarithm) as a function of the exposure-time; it is then easy to calculate the exposure-time for which *Polaris* would have given the same impression as the exposure on the sky. The time thus determined differs very little from 10 minutes; it is legitimate to apply the law of reciprocity for times differing so little.

Remarks.—The absorption by the lenses can be disregarded, for the light always passes through the same apparatus. It is true that, in the exposure on the comparison star, the light passes through the central part only of the microscope objective *B*, while, in the exposure on the sky, a part of the light passes through the edge; but the thickness of this objective is so little that the error which results ought to be negligible.

There enters into the calculation only the angular diameter of the circle of which the image is projected on the diaphragm; it is sufficient to know the focal distance *F* of the telescope objective *A*, and the diameter *d* of the diaphragm. The focal distance of the microscope objective does not enter. It is not necessary that this objective should have very good optical qualities; however, the one which I have used, made of two spectacle lenses, was perhaps a little too far from perfection.

The measure gives the mean intrinsic brightness of the portion of the sky whose image is in the aperture of the diaphragm; that is, in my apparatus, a circle of $3\overset{\circ}{.}5$ diameter. It is evident that if there is a bright star in this circle, the value found will not have any relation to the mean brightness of the sky. This is, however, easily avoided. Furthermore, it is easy to give to the apparatus slight irregular displacements around a mean position during the exposure, and thus obtain the mean intrinsic brightness in a more extended region.

Results.—I have made measures only in two regions of the sky.

A. The region near the celestial pole, near the star ϵ *Ursae Minoris* (but not including this star), in galactic latitude 30°. I find that the photographic intensity of one square degree = 0.103 of that of *Polaris*.

B. The region between β and γ *Cygni*, one of the most brilliant

of the Milky Way: photographic intensity of one square degree = 0.212 of that of *Polaris*.

According to King,[1] the photographic magnitude of *Polaris* ought to be considered as 2.62, if it is desired to make photographic and visual magnitudes coincide for the white stars (spectroscopic class A).

Using this value, we find: (1) Region of *Ursa Minor:* photographic intensity of one square degree = 0.92 star of 5th photographic magnitude; (2) Region of *Cygnus:* photographic intensity of one square degree = 1.90 stars of 5th photographic magnitude.

Comparison with the results of Newcomb and Burns.—From the point of view of relative intensities of different parts of the sky, my result is entirely in accord with the visual measures.

In absolute value, the values which I find for the photographic magnitude are smaller than those obtained visually. This naturally follows from the manner in which the scales of magnitudes have been chosen: the number 2.62 adopted for the photographic magnitude of *Polaris* is assigned so that the white stars (type A) should have the same photographic and visual magnitude; as it is certain that the mean color of the stars is more red than that of stars of type A, the photographic intensity ought to be less than the visual intensity. If we should adopt a photographic scale such that *Polaris* should have a photographic magnitude equal to its visual magnitude (2.12), we should find: one square degree in the region of *Ursa Minor* = 1.46 stars of magnitude 5, a result between those of Newcomb and Burns.

We may say then that, within the limits where agreement was possible, my results are in accord with those of the visual measures.

Comparison with the statistical data.—1. E. C. Pickering[2] has found that in the Milky Way the stars of each magnitude are twice as numerous as in the non-galactic sky. The intrinsic brightness of the sky would, therefore, be twice as great in the Milky Way as elsewhere, a conclusion agreeing with the result of the direct measures.

On the other hand, Pickering gives a table of the number of stars of each magnitude in the whole sky. We may deduce from it the total (visual) intensity of all of the stars of the sky. We thus find:

[1] *Harvard Annals*, **59**, No. 4.

[2] *Ibid.*, **48**, No. 5.

Visual intensity of all the stars = 306 stars of zero magnitude, and, consequently,

Mean intensity of one square degree = 0.74 star of magnitude 5.

As this number represents the visual intensity, and as the galactic region enters into the mean, it is probable that it is at least twice too small.

It is true that the table of Pickering does not include stars below magnitude 13.5; but, according to his opinion, the fainter stars would add very little to the total intensity.

2. The remarkable investigations of Kapteyn[1] lead him to results very different from those of Pickering; he gives a much greater importance to the very faint stars, and finds a distribution much more variable as a function of the galactic latitude. Here are some of his results:

Galactic Latitude	Visual Intensity by Square Degree
0°	7.42 star of magnitude 5
30°	1.26
90°	0.48

The number for latitude 30° ought not to be far from the truth; the result for latitude 0° is probably more than two times too great, and that for the galactic pole probably more than two times too small. It appears that Kapteyn has considerably overestimated the influence of the Milky Way.

For the celestial sphere as a whole, Kapteyn finds a visual intensity equal to that of 2384 stars of magnitude 1, which gives:

Mean visual intensity of one square degree = 2.3 stars of magnitude 5, a result which is probably not very far from the exact value.

Upon the whole, therefore, the data actually obtained on the number of stars are very far from being in accord with the result of measures of intrinsic brightness. Is the disagreement a result only of the inaccuracy of the measures and of the statistical data? That is not certain. If it were proved that the total intensity of the sky exceeds considerably the sum of the intensities of the observable stars, one could advance two hypotheses: either that there exists an immense number of stars too faint to be observed with our instru-

[1] *Publications of the Astronomical Laboratory at Groningen*, No. 18, 1908.

ments, or that there exists throughout the sky a sort of continuous nebulosity giving a uniform brightness. The statistical results do not appear certain enough, up to this time, to warrant the adoption of either of these hypotheses. If it should become necessary, it would be interesting, and perhaps not absolutely impossible, to obtain the spectrum of the total light of the sky.

Remarks bearing on further investigations.—The method described above gives, without much trouble, precise measures, but my results are very incomplete. It would be necessary to extend them over a great many more regions of the sky. I have no intention of doing it: in astronomy I am only an amateur; the measures can be made only very far from cities, and the results given above are only those of work done in vacations. A few evenings of observations in an observatory would give much more complete results. I wish simply to indicate what it would be possible to do.

Let F be the focal distance, and D be the aperture of the telescope objective A; f the focal distance of the microscope objective, and d the diameter of the diaphragm which is used in the exposure on the sky.

It is necessary to make two exposures: one, on the region of the sky to be studied, with the diaphragm d; the other, on the comparison star, with a very small diaphragm. This second exposure might be omitted, if one wished only to compare the different portions of the sky.

In the exposure on the sky with the aperture d, the time of exposure necessary to obtain a satisfactory photographic impression depends only on the ratio d/f; this time is independent of the telescope objective A (if one neglects the absorption), and increases as $(f/d)^2$. For the non-galactic sky, with Lumiere "Sigma" plates, the time of exposure would be about $(f/d)^2 \times 10$ minutes.

I have chosen practically $\dfrac{d}{f} = 1$. The use of a smaller aperture would necessitate longer exposures, which would make the observation more difficult and increase the chances of atmospheric variations. Conversely, it is possible to combine optical systems having a ratio d/f greater than 1 (this is the case in objectives of microscopes), and then make very short exposures, but the marginal rays would strike

the photographic plate very obliquely, which would introduce an error in the comparison of the sky with a star. This difficulty would not exist in the comparison of different regions of the sky (employing diaphragms in the form of sectors), and then comparisons could be obtained with exposures of a few minutes.

The luminous spot, the image of the telescope objective, which is projected on the photographic plate, is a circle whose diameter δ is given very nearly by the equation $\delta = D\frac{f}{F}$. This image should not be too small, and this condition, with a given telescope objective, would fix a minimum for the focal distance f of the microscope objective. In my apparatus, $\delta = 3$ mm, which is more than ample for measures of opacity. It is probable that an image of 1 mm would be sufficiently large.

The measure gives the mean intrinsic brightness of a portion of the sky bounded by a circle whose angular diameter is $\frac{d}{F}$ radians. It is possible, at will, to measure a very small portion of the sky, or a very large portion. The comparison star will need to be chosen accordingly. Its photographic magnitude will be about

$$m = 5 \log\frac{F}{d} - 3.4 .$$

I give below two examples, covering two extreme cases.

1. If it is desired to measure the mean intrinsic brightness on a circle of large radius, take a telescope objective of short focus, and a diaphragm of very large aperture. As the telescope objective can in this case have a small aperture, it will give a sharp enough field. Take, for example:

Telescope objective, $F = 13$ cm, $D = 0.4$ cm;
Microscope objective, $f = 3$ cm, $d = 3$ cm.

The comparison star will be of magnitude 0, the exposure will be 10 minutes, and the image on the photographic plate will have a diameter of 1 mm. The measure will give the mean intrinsic brightness of the portion of the sky inclosed in a circle 13° in diameter.

2. The mean intrinsic brightness can be measured in a circle having a diameter of a few minutes of arc, or even one minute, by

taking for the telescope a large objective of long focus, and as microscope objective a system of very short focus. For example:

Telescope objective, $F=20$ m, $D=1$ m;
Microscope objective, $f=10$ mm, $d=6$ mm.

The photographic image will be 0.5 mm in diameter; the comparison star will be of magnitude 14, and the exposure-time about 30 minutes. The measure will be of a circle 1′ in diameter. On such a small surface it will be easy to compare the total luminous intensity with that of stars really observable, to compare, to some extent, what one sees with what one does not see. A photographic impression might be obtained which was produced solely by invisible stars, concealing with screens the visible stars.

Finally, it would be easy to secure an idea of the color of the light of the sky as a whole, by making exposures with ordinary plates and with orthochromatic plates through suitable screens. The use of microscope objectives of large angular aperture would make moderate exposure-times possible, without introducing any cause of error in this special application.

Application of the method to stellar photometry.—The method which I have described in this paper might perhaps be useful for the photographic comparison of stars. In projecting, as I do, an image of the telescope objective on the photographic plate, a circle of uniform illumination is secured, which would not appear to be always the case with extra-focal star-images. It would not be necessary to have a microscope objective of large angular aperture, but it would be convenient to have one whose focal distance could be varied gradually. This could easily be arranged by making it of two parts whose distance could be varied.

University of Marseilles
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ADDENDUM

After the present article had gone to the printer, two papers on the same subject came to my attention.

In 1903, Sydney D. Townley[1] employed, to measure the intrinsic brightness of the sky, a photographic method very different from

[1] *Publications of the Astronomical Society of the Pacific*, **15**, 13, 1903.

that described above. Without interposing any lens, he compared the photographic impression produced by *Vega* with that of a convenient portion of the sky. The exposure-time necessary was one hour. He found that a square degree of non-galactic sky is equivalent to a star of 4.5 magnitude; and that the brightness of the galactic sky is 1.9 times greater.

Very recently, Yntema[1] has made a great many visual measures. The most important conclusion from his observations is that a great part of the light of the sky is of terrestrial origin. This result seems to be peculiar to the conditions under which Yntema has observed. No other observer has found, from one date to another, such large variations as those observed by him. The brightness of a square degree in the region of the celestial pole would equal 0.19 that of a star of magnitude 1, a result about four times as large as those found by other observers.

The only conclusion to be drawn from these great discordances is that measures made in different epochs and in different places would be very useful. The method I have described would permit of their being made with little difficulty, and with apparatus very easy to transport.

[1] *Publications of the Astronomical Laboratory at Groningen*, No. 22. See also Gavin J. Burns, *The Observatory*, March and April 1910.

STUDIES ON THE EMISSION OF GASES

I. MEASUREMENTS ON THE INTENSITY AND ENERGY IN SPECTRA

By H. KONEN and W. JUNGJOHANN

We report here on a series of experiments and measurements made by us, partly in collaboration and partly separately, and in part with Mr. J. Kyll, in the attempt to contribute to the solution of the questions: What are the factors which determine the emission of gases? and, Is it possible to separate these factors without giving the hypothesis so wide a field as has commonly been the case?

We shall begin in this first paper by presenting the points of view which may be taken in regard to the measurement of energy in gaseous spectra. A later paper will treat of the properties of the different sources of light we have employed, and a third will state the results of photometric measurements. Further communications will follow these in close succession.

1. In contrast to the case of the emission of solid bodies, the investigation of the emission of gases has hitherto not led to unambiguous quantitative results of general validity, if we leave out of account the relation between the wave-lengths in series-spectra and similar spectra. The separate qualitative results on the variation of spectra are indeed exceedingly numerous and varied. We shall, further, not discuss in detail the numerous theories of gaseous emission. It may be asserted that no one of the theories or hypotheses is able to include the wealth of facts; still less to make quantitative predictions without the assistance of an excess of "guesses" (*Vermutungen*). The reason for this unfortunate condition is partly to be sought in the fact that the available data are comparatively few for quantitative investigations as to the variability of spectra and the comparison between the variable data and the variables. Pressure-shift, the Zeeman effect, dispersion, magnetic rotation, measurements of energy in emission and absorption with different variables are the principal topics for which numerical results are available. We shall occupy ourselves here with the last-named topic. The number of papers

dealing with it and with the phenomena underlying it is already very considerable. Kayser's *Handbuch der Spectroscopie* gives a survey extending to the year 1902. We have collected in the footnote the papers appearing since 1902, and extend the list by the papers already named by Kayser. We name in the first group (Nos. 1 to 30), exclusively those papers in which are treated either the relative changes of intensity in spectra or measurements of emission; in the second group (Nos. 30 to 36) those in which the applicability of Kirchhoff's law to gases is quantitatively treated, or in which an attempt is made to determine the temperature of gases, for the most part flames, on the basis of a comparison of the emission of gases with that of solid bodies. The third group (Nos. 36 to 41) contains the papers in which the absorption of luminous gases is investigated; some of the papers from the first group also belong in this group. An extended analysis and criticism of all these papers may be found in a paper by Mr. Kyll soon to appear.

2. A comparison of the results of the different investigations indicates only slight agreement. Precise investigations in this respect have been made only on the spectra of mercury and of hydrogen, but there are also numerous, though less exact, researches on the band spectra of nitrogen.

For the latter, most observers state that the intensity of the bands increases proportionally to the current-strength. For mercury, there are precise investigations by Küch, Retschinsky, and Pflüger, from which it follows that the emission, as well as the absorption, increases with the increasing number of watts used in the mercury lamp, and, within any series of lines, the more rapidly for the lines of short wave-length. The change of emission is different for each line within the separate triplets. The absorption, further, does not proceed parallel to the emission. All this points toward a shifting of the maximum of emission toward the blue end with increasing temperature, within each series of lines, which is similar to the shifting of the maximum of a black body. Furthermore, in the case of hydrogen and a few other gases, a similar shifting of the maximum toward the blue end was first proven by Langenbach, or a more rapid rise for the short wave-lengths; from which Kayser was in fact able to compute a plausible value of the temperature of emission based upon the rules

for black bodies. Finally, the determination of the temperature of flames by experiments on reversal with the aid of black bodies has led to temperatures which are in agreement with the results of other measurements. It would appear from all this as if the previous experiments indicate that a curve of emission could be drawn for each series that is similar to that of the black bodies.

A careful comparison of the different experiments shows, however, that only the measurements of mercury vapor have been executed with the necessary precision and embrace a sufficient range of current. The measurements with hydrogen, as well as numerous ones of nitrogen, were carried out with discharges from condensers, so that only mean values could be observed. But the measures on nitrogen with a direct current have led to contradictory results, very small currents having been used for the most part. Geiger had indeed employed stronger currents, but he took into account only the strength of the current and not the density of the current. The investigations by him and others are contradictory in so far as it was found that the total color of the discharge changes while the intensity of each region of the spectrum is proportional to the current-strength. It should be further mentioned that other observers found linear relations.

Finally, as is well known, opinions are divided as to whether temperature is after all to be regarded as the cause and efficient variable in gaseous emission. On the one side, the possibility of bringing gases to luminosity by mere elevation of temperature is totally denied. On the other side, the astrophysicists are constantly figuring with the temperature of the luminous gases. We must here pass over the arguments adduced by the two sides, as well as a detailed discussion of the different experiments. It is clearly evident from what has been said that no agreement prevails either as to the change of emission with the current-strength or temperature, or even with the relation of the latter to the observed changes. This holds good above all for the band spectra, which have been only imperfectly investigated in comparison with the line spectra.

It therefore seems to us useful at first to summarize the ideas fundamental for measurements of the sort described, and to do this with the use of the facts already printed in the literature. New facts are not given therewith; but our remarks are intended merely to

illuminate the importance of the conclusions drawn, and to avoid statements that are not clear. We shall here not give references to the literature at every point.

3. If the variable factors of a gaseous spectrum are to be measured, then the nature of the dependent and the independent variables must first be established. The definition of both of these is beset with great difficulties.

The intensity or the energy of a definite point in the spectrum is commonly designated as the variable quantities here in question. The former is measured in comparison with a constant source of light, and serves for comparing the relative changes within a spectrum. The latter is either measured directly, as by the bolometer, or is derived by comparison with the intensity in the spectrum of a body whose distribution of energy is known. In both cases, principally in the latter, there have to be applied numerous corrections which are in part uncertain. Preference should be given to the determination of the energy of one point in the spectrum when this can be executed.

But with the quantities defined in this manner nothing can be done without further consideration. In the first place, practical difficulties are to be considered. It is indeed possible, in the case of some spectra having few lines, to measure the separate lines with the bolometer or photometer. This procedure is, however, in general excluded in the case of spectra having numerous lines, or in band spectra. For instance, the positive bands of nitrogen are only wholly resolved in the higher orders of large Rowland gratings, and the same is true of most other bands. It may be that intensities of separate components can be measured in a photographic manner, but until that can be done, it is only possible to measure by the photometer or bolometer average values of the energy or intensity, the interpretation of which depends upon the variation of each separate component, and the magnitude of which depends upon numerous subordinate factors, such as the position of the center of gravity of the band, the width of the slit, etc. But that changes of the components or of the position of the center of gravity may occur is proven by the investigations on the minimum in the cyanogen band λ 3884, and on the variations of intensity within the separate series of the nitrogen

bands. This is a circumstance which, as it appears to us, has been considered by none of the numerous observers who have investigated the nitrogen spectrum.

The same thing holds good in less degree for the line spectra. Cases are known in which the relative intensity of the components of compound lines changes simultaneously with the intensity of the excitation of the vapor. Further, it is practically impossible to produce a homogeneous stratum of luminous vapor. Therefore phenomena of broadening and reversal are observed, and this particularly with intense excitation. Both of these falsify the measures, the first particularly the photometric measures, the latter affecting all kinds of measures. Since in the photometry of lines a certain minimum width of slit must necessarily be employed, the assumption must be made, in absolute measures, that the brightness is independent of the slit-width employed. In relative measures it must be assumed at least that the dependence of the brightness upon the slit-width does not vary with the excitation of the luminosity. But if a broadening of the lines occurs, the assumption thus made will not apply. An investigation on this point is thus at least necessary to determine how far this is a fact. We shall very presently discuss the effect of the phenomena of reversal.

4. To the practical difficulties named are added those of principle. The absorption must be taken into account in addition to the specific emission of gases. If this is very small, the intensity increases approximately proportional to the thickness of the stratum, without alteration of the relative intensity of the lines or bands. But if, on the contrary, it does not vanish, then the brightness rises toward a maximum with increasing thickness of stratum. At the same time the relative intensity of the separate lines may change because of a difference in the absorption. Added to this is the fact that the absorption, for itself, is again a function of the variables upon which the emission depends.

If we could directly assume the validity of Kirchhoff's law of gases, this difficulty would be overcome; but this is by no means the case; examples in which the validity of the law is probable are contradicted by others in which, in spite of the strongest excitation, it has not hitherto been possible to prove an absorption.

In any case, every measurement of the energy in a discontinuous spectrum must be supplemented by a measurement of the absorption, even when relative measures only are made, and the thickness of stratum remains constant. This last point cannot be rigorously realized, however; for instance, if a gas is rendered luminous by an electric current, it is impossible to avoid having less highly excited strata of gas at the ends of the apparatus used. Their emission and absorption is in general different from that at the portions where the gas is most strongly luminous. Their size also changes with the amount of the excitation, so that in very many cases self-reversals are obtained, which are recognizable only when high dispersion is employed. No general statement can be made as to the magnitude of the error thus introduced, but it must be determined in every individual case.

5. Furthermore, the case often occurs that the spectrum to be investigated is a mixture of different spectra, e.g., of a continuous spectrum, a band spectrum, and a line spectrum. This introduces a double difficulty: (1) the share of the change in energy found for a definite point of the spectrum must be determined for each of the components, which is not always possible. An example is presented by the spectrum of mercury in a quartz tube through which mercury vapor is distilled: the energy of the lines λ 4348 and λ 4349 can then be measured only by uncertain indirect methods. Still greater, however, is (2) the difficulty thereby introduced, because with the change in excitation of the gas not only does the intensity at every separate point in the spectrum change, but also the composition of the spectrum is altered. For instance, if we are dealing with the superposition of a band spectrum upon a line spectrum, then not only will the intensity of both vary, but also the ratio of the intensity of the two. This effect is not limited to different classes of spectra, but even occurs between lines of one and the same spectrum. It may be asserted with a high degree of certainty that the number of emitting particles in a gas at a given time is small in percentage, and similarly that the different emissions correspond to different conditions of the emitting particles. On this assumption, the effect named may be described by stating that the number of particles producing a definite line shares in determining the intensity of the emission. According as this

number increases or decreases under given conditions, an increase or decrease of the emission may be observed at a definite point in the spectrum, although perhaps the mean emission of each separate particle has changed in the opposite sense.

Therefore, if we wish to obtain values of the emission in order to make a comparison for a definite point in the spectrum, or also for two different wave-lengths, then we must either determine the quantity of the luminous gas or eliminate it. If we wish to institute a comparison between different wave-lengths, then we must be certain that the waves in question have the same centers of emission. Kayser was the first to point out the decided importance of this point, which has often been overlooked in recent publications. The decision whether the same centers of emission can be assumed for given wave-lengths is often very difficult. All the facts must be taken into account which can give any hint as to the nature of the luminous particles. As was remarked first by Kayser, we may regard the fact that they belong to the same series as a fair sign of the same origin for different lines, since the simultaneous appearance of the lines of a series, and their similar behavior under the influence of pressure, of a magnetic field, and of other physical factors, as well as the relation between their vibration numbers, makes it in the highest degree probable that the lines in question were produced by the same vibrating system.

But there are still other cases in which we may assume the same thing; for instance, the series within one band of a band spectrum, which may be included by a formula similar to those of the series; or absorption lines which furnish the same series of fluorescence lines. The possibility is also not excluded that we may regard the whole system of bands as of the same origin. Dispersion, or the separation of luminous masses of gas in space, or in electrical or magnetic fields, in short all the physical factors, must be taken into account which characterize a definite spectral line in order to decide whether or not we may assume the same origin for given spectral regions.

6. The suggestion is obvious that instead of the emission or the absorption of a gas, we may introduce, as the variable to be measured, the ratio of the two, after the analogy of Kirchhoff's law. This

proposal has already been made,[1] but whether it may be adopted, and whether the resulting function, as the ratio of emission and absorption for each group of lines, follows simple laws and is independent in respect to its form from the nature of the instantaneous centers of the emission, must certainly be determined by experience and is not a priori obvious. The chain of reasoning on which is based the derivation of the Kirchhoff law and of the radiation function of the black body fails to apply to gases, as is well known, unless certain hypotheses are made. In the first place it must be assumed that the luminous gases absorb the same kind of light that they emit. Although this has been measured for different examples and is proven to be qualitatively applicable for many lines of the flame, arc, and spark, by experiments in reversal, the general proof has not been given. The opposite is expressly asserted for single examples, as the band spectra of many flames; and we cannot a priori exclude the possibility that in gases similar phenomena may occur as for fluorescent bodies, for which hitherto it has been impossible to prove the existence of a maximum of absorption corresponding to the emission, and where emission and absorption appear to belong to two different states of the luminous particles. Decisive experiments on this point are still awaited.[2]

But even if we assume that in every case a gas absorbs the identical wave-lengths which it emits, it nevertheless remains certain that in many cases the absorption is exceedingly small and below the limits of measurement. Then, if D represents the thickness of the luminous stratum, E the emissive power of an infinitesimal stratum of thickness dx, and a the coefficient of absorption, the emission of the gas for a wave-length λ will be

$$\epsilon=\int_{O}^{D} e^{-ax}Edx$$

or practically equal to DE. We therefore determine E only. For an infinitesimal stratum, ϵ must become equal to the emission of

[1] See A. Pflüger, *Annalen der Physik* (4), **24**, 575, 1907; also the article "Theorie der Strahlung," by W. Wien, Bd. 3, Heft 2, *Encyklopädie der mathematischen Wissenschaften*, p. 348, Leipzig, 1909.

[2] Kayser, *Handbuch der Spectroscopie*, Bd. 4, p. 964, where one of us discusses the matter fully.

the black body. But it will not always be possible to realize in practice an infinitely thick stratum by magnification of the thickness of the stratum or by applying mirrors in the proper way.

This indicates that in most cases the relation of the emission to the absorption cannot be determined at all, and we are compelled to draw our conclusions from the behavior of the emission only. But we must still, with Pflüger,[1] regard the measurement of the ratio $E:A$ as one of the most important problems in the quantitative investigation of gaseous spectra.

7. Finally, we must give some consideration to the question as to how far a spectral line is characterized by the data as to its emission and absorption, first leaving out of consideration all the other physical data which are significant for a line, as the Humphreys-, Zeeman-, and Doppler-effect, etc., and considering only the influence of lack of homogeneity. Every line of a band or line spectrum is to be regarded as practically a portion of a continuous spectrum, provided it does not have satellites. It is not known whether the light contained in it is equivalent to a portion of the spectrum of a black body in the same spectral region in this respect, but we can in any case assume, with Wien,[2] that this is so. There are, nevertheless, the greatest differences in respect to lack of homogeneity between different spectral lines: in the same spectrum, e.g. the spark spectrum of a metal, there occur extremely diffuse lines alongside of very sharp lines. Therefore, if we make our measurements with instruments of small resolving power, as a bolometer, then the total energy contained in the line will also be measured at the same time; hence it can happen that a diffuse line gives the same energy as an intense, but very sharp, line. If we measure with the eye, then, as already mentioned, it depends on the instrumental conditions whether we obtain a quantity nearer to the total energy of the line or to that of a definite wave-length. Finally, if the measurements are made photographically, we obtain primarily from the degree of blackening the intensity referred to a definite wave-length. The attempt has recently been made to derive from this value the total energy of the line by multiplying by the breadth of the line. (See below.)

[1] *Annalen der Physik* (4), **24**, 515, 1907.

[2] *Op. cit.*, p. 348.

It is in general customary to regard the total energy in a line as characteristic of it. This leads, however, to some consequences of little plausibility: it does not agree with direct observation to call a broad, diffuse, and often hardly visible strip in a spectrum equal to a bright, strong line. This view proves to be properly established in certain respects; it depends on what we measure as the intensity of a line. If we start from the view that energy is contained in the broadening of a line which becomes appreciable as an increase of the total brightness of the luminous gas, then we are embracing the total emission of the luminous system under investigation. We must then integrate over the entire width of the line in question. But to the value thus obtained we must add the contributions of energy from all the other lines which we ascribe to the same center of emission; only in this way can we actually obtain the total emission, which we may then compare with the total emission of other centers or of solid bodies. But in practice we are concerned with something else: a measure of the energy in the sense of the optical intensity is desired; or the line in question is compared with a continuous spectrum under equal resolution. Then the matter depends on whether or not the maximum of the line for a given apparatus exceeds the intensity at the same place of the comparison spectrum. The ideal procedure would, therefore, be to determine the intensity- or energy-curve within each line; but as this is generally not possible, the attempt will be made to determine a quantity proportional to the maximum of the line. That it is generally this quantity, and not the integral, which is involved, appears from the fact that the experiments of the reversal and the comparison with the black body for fixed wave-lengths depend upon it.

We therefore propose to differentiate between:

a) The total energy of a line, which is obtained by integration from the energy-curve of the line; this value can be employed immediately only in the case where the luminous system is emitting this one line only.

b) The total energy of a system of lines; this is equal to the sum of the total energies of all the lines connected in the system (e.g., the lines of a series).

c) The intensity of a line; this is equal to the maximum of the

energy-curve of the line. If the energy-curve of a line cannot be obtained, then it is necessary to give the data (*c*) along with the mean value of the total energy of a line.

Similar statements may be made as to absorption.

8. A thorough discussion of the variables which condition the distribution of energy in the spectrum of a gas is not possible without taking up the numerous hypotheses as to the nature of gaseous emission. We therefore content ourselves with mentioning a few of the principal points; and we here would by no means claim to bring forward anything new. The purpose of our remarks lies in making the most precise statement possible of the assumptions in measurements of intensity, with the avoidance of all unnecessary hypotheses.

The variables upon which a gaseous spectrum depends differ according to the source. In an electric furnace, such as used by King, temperature is the primary variable; in the electric arc there come into question current-strength, potential-gradient, chemical processes, surrounding atmosphere, electrical data for the circuit, character of the electrons, pressure, quantity of vapor, effect of other elements present as impurities, and perhaps still other factors, which vary from point to point in the arc. With the oscillating spark there are added to all these factors the time of the observation (order of the phase of partial discharge). In flames the most different kinds of chemical processes play a rôle, along with the temperature, the pressure, the quantity of material, and such factors. For discharges in Geissler tubes all of the items named for the arc and spark occur together, and to these in all cases are added the pressure of the gas under investigation, and the degree and character of its ionization.

Among the variables thus mentioned, there are doubtless many of a secondary nature, but it is not decided to what degree this occurs. It is assumed by many that with constant pressure the temperature is the controlling variable. This is not to be measured as the average temperature of the gas, but refers principally to the luminous particles, indifferently whether these are assumed to be charged or not. The mean internal energy is supposed to depend on the temperature of the centers of radiation, and upon these again depends the energy of the radiation. Whether the inner energy of the radiating parts

sustains a simple relation with the temperature in an ordinary sense remains uncertain. But in any case, it seems possible that this may be so, and, further, that there exists a simple relation between the internal energy and the radiated energy, or also between the so-called temperature determining the inner energy and the radiated energy; in form, this relation may agree with similar laws of a black body. Chemical processes and ionization are then regarded as also determined by the temperature. But the temperature of the luminous centers in question, which possibly constitute but a small fraction of the gas, is not directly measurable, since all measurements furnish only a mean value. Therefore, even if we adopt the above point of view, we may not employ the temperature of luminosity as a variable; the converse process only can be employed. We may measure the change of energy in a spectral system (e.g., a series) and from the change, on the basis of some definite hypothesis, such as the assumption that the same rules hold good as for a black body, compute a temperature, the validity of which may be tested in some other way. This procedure was first carried out in logical manner by Kayser.

9. A modification of the view given here is to regard the temperature as the variable in the ordinary sense, at least in many cases. The temperature of luminosity of a radiating gas would then be defined as that temperature at which a black body is in radiation equilibrium with the gas in question. If we overlook entirely the fact that the proof must still be produced that a radiation equilibrium of this sort is possible, the objection may be made that practically in many gases the temperature of the gas could not be measured, because it would not be possible to produce a black body of sufficiently high temperature. Nevertheless, noteworthy results have been obtained on this assumption, and we intend to return to this point again.

10. In many cases it is not possible to produce in gases heated in closed receptacles the emission of spectra readily evoked in other ways; further, the luminous gases are in many cases appreciably ionized; finally, it can be proven that in many instances the ions are luminous; hence it is natural to think that the essential element in the emission of gases is to be found in this ionization, whether the emission is attributed to the recombination of ions or electrons, or to perturbations which the ions suffer when they are free. From

this point of view the temperature plays a secondary rôle: it determines under some circumstances the degree of ionization, and perhaps also the amount of excitation of the ions. We must expect that the factors determining the ionization, namely, current-strength, potential-gradient, position in the circuit, and chemical processes, play the principal rôle and determine the emission of the gas. This view has in its favor the fact that certain phenomena in vacuum tubes, also of the relation between ionization and luminosity, as well as the dominance of the chemical and electrical methods in the production of gaseous emission, are explained in a simple manner. It further permits us to include all the phenomena observed in the electrical conductivity of gases, and it is very capable of adaptation. We have a large number of possibilities of combination, and may regard the positive ions, or the negative ions, or modifications of them or the processes occurring in the formation or recombination, and similar effects, as determining the emission. But great disadvantages are opposed to these advantages: first, it is in no wise proven that in all cases the luminous gas is ionized, or, conversely, that any ions present are the carriers of the emission. We should first name here numerous spectra of compounds for which this proof is conspicuously lacking.

Even if we adopt the standpoint thus described, the practical application of the assumption in most cases goes to pieces from the complication arising from the many kinds of ions. For instance, in the case of helium, we should have to assume that there are at least six different kinds of ions, or perhaps as many different processes of excitation; and for elements having many lines these would be still more numerous. But this excludes the ionization as the independent variable, so long as it is impossible to separate and investigate the different kinds of ions. The assertion may be maintained that it has not hitherto been possible for anyone to isolate in an unquestionable manner ions or atoms of the same gas with emission proven to be different. Numerous examples have indeed been given from which such a conclusion has been drawn, but there have always been valid objections to these. The ionization of a gas would be practically treated as the variable only if the total emission in a given space, e.g. in the positive column, could be referred back to a single kind of ions (or process of ionization). In this case different conse-

quences would follow, to which we shall revert. It is obvious that these last considerations find their particular application in gases through which a current is passing, but first of all a relationship would have to be found between the number of ions and the energy emitted. It has already been asserted on the basis of such a process of reasoning that the emission must be directly proportional to the number of ions, hence to the strength of the current. But the objection may be made to this that the intensity of the excitation will change simultaneously with the number, if we may indeed assume that the luminous particles are capable of different degrees of excitation, which assumption would include a sort of temperature effect. In that case, we should select as the variable, not the current-strength, but rather the work done, hence the potential-gradient times the current-strength. This proposal has already been made, but without being logically carried out.

11. To the variables thus named there must be added, as a subordinate variable, the pressure of the gas, which plays a large rôle, particularly in electrical methods. Its greatest effect is due to the change of the discharge, and may be explained from the standpoint of the theory of the electrical conductivity of gases, if we regard either the temperature or the ionization and related processes as variables; but many experiments have shown that there are attendant phenomena pointing to a direct influence of the pressure on the emission: we mention here particularly the phenomena of broadening, and we shall revert to this point. Experiments on the emission of gases are greatly hindered by the condition that the pressure should be kept constant. This applies to the mercury lamps which are otherwise so excellent. Although experience shows that the pressure of a gas has the greatest effect on its emission, we cannot say a priori that the total pressure of the gas is the independent variable; it is thinkable that only the partial pressure of the luminous constituents was effective.

12. The different assumptions we have mentioned by no means exhaust the variables determining the luminosity of gases. It appears quite thinkable that there are different classes of emission, or that in individual cases several of the causes named operate together; but we abstain from depicting the possibilities that would thus arise.

We can decide between them only when it has been established by numerous examples what is the actual relation of the previously defined dependent and independent variable quantities, and whether the variation of the former can in any wise be satisfactorily represented with any of the latter. In a later communication we shall first report upon experiments of this sort, which seem to indicate that the relations are complicated, by finding different variables for different spectra.

MÜNSTER
March 5, 1910

NOTE ON THE INTERPRETATION OF SPECTROHELIOGRAPH RESULTS AND OF LINE-SHIFTS, AND ON ANOMALOUS SCATTERING OF LIGHT

By W. H. JULIUS

The puzzling character of solar problems is well illustrated by the fact that the images obtained with the spectroheliograph give rise to widely different explanations, and that it seems impossible as yet to answer in a satisfactory way even the fundamental question: What is the principal cause of the very unequal distribution of different kinds of light over the sun's disk?

Hale and Ellerman, in a paper "On the Nature of the Hydrogen Flocculi and Their Structure at Different Levels in the Solar Atmosphere,"[1] reject the hypothesis advanced by W. J. S. Lockyer, that the dark hydrogen flocculi indicate regions where there is a deficiency of hydrogen. They also refute Deslandres' argument, according to which those dark flocculi are not mainly due to a particular distribution of the emissive or absorbing power of hydrogen, but to a simple instrumental cause, an inherent defect of the spectroheliograph. In their own opinion, the best way to account for the observed phenomena is the hypothesis that the dark hydrogen flocculi are produced by increased absorption (probably resulting from greater depth and decreased temperature of the hydrogen gas in these regions of the solar atmosphere), while the bright flocculi represent regions of increased radiation. Finally Hale and Ellerman state that the results obtained in the high-dispersion work with the hydrogen lines are also in accord with certain inferences which I deduced[2] from the hypothesis first advanced in 1904,[3] that the distribution of the light in photographs taken with the spectroheliograph is mainly

[1] Hale and Ellerman, *Proceedings of the Royal Society*, **83**, 177, January 1910.

[2] Julius, "Anomalous Refraction Phenomena Investigated with the Spectroheliograph," *Contributions from the Mount Wilson Solar Observatory*, No. 29; *Astrophysical Journal*, **28**, 360, 1908.

[3] Julius, "Spectroheliograph Results Explained by Anomalous Dispersion," *Proceedings of the Royal Academy of Amsterdam*, **7**, 140, 1904; *Astrophysical Journal*, **21**, 278, 1905.

caused by anomalous dispersion. They wish to defer a general discussion of the effects of anomalous refraction in the solar atmosphere until many more observations have been made; but a preliminary survey of the results already obtained induces them to believe that the principal phenomena of the dark flocculi may be explained more satisfactorily as absorption effects, and that the evidence can hardly be considered favorable to my theory.

In the present note I wish not to combat the absorption hypothesis proposed by Hale and Ellerman, but only to show that their objections to an explanation on the basis of anomalous dispersion are easily refuted, and that the results so far obtained are by no means less favorable to the theory which ascribes the flocculi in the main to anomalous dispersion, than to that which explains them as mere absorption effects.

The intensity and width of the hydrogen lines, especially of $H\alpha$, differ greatly in different regions of the sun. If the widening of these lines is caused by increased absorption only, there is no reason to expect them to be asymmetrical (except perhaps by local displacements in consequence of motion in the line of sight). If, on the other hand, we are chiefly dealing with dispersion bands,[1] enveloping the real absorption lines, so that the widening results from the fact that the strongly refracted waves bordering the central lines have their origin on the average in less luminous regions—then, at first sight, it seems as if a marked and variable asymmetry must be the general appearance. Indeed, when comparing waves at equal distances from the center of the line on the red and violet sides, we must find the rays curved in opposite directions by the same density-gradients of the solar atmosphere. Hale and Ellerman think it improbable that equal amounts of light would reach the observer in both cases, and therefore conclude that, if anomalous dispersion were the principal cause, the spectroheliograph should, as a rule, give very different images when set on the one or the other side of the same line.

They tried the effect of photographing the flocculi simultaneously with light from opposite sides of the $H\alpha$ line at equal distances from the center. In general the two images proved to be almost identical in their principal features, though small differences of detail were

[1] Julius, *Astrophysical Journal*, **21**, 271–291, 1905; **25**, 95–115, 1907.

often visible. In the case of what they call "eruptive phenomena" the images were very unlike, as the distortion of the $H\alpha$ line would lead one to expect. It stands to reason that, if such distortions are satisfactorily explained on the basis of the Doppler effect, a corresponding explanation can be given of the unlike parts of the images just mentioned.

These are the considerations adduced by Hale and Ellerman in support of their conclusion that the results hitherto obtained are unfavorable to the anomalous dispersion theory.

On closer examination, however, the consequences of anomalous dispersion turn out to be in harmony with the observed phenomena. It will indeed prove very probable that R-light and V-light[1] selected at equal distances from $H\alpha$ should, in spite of their opposite curvature, produce images which are in general almost identical in their principal features, and that differences of detail should chiefly appear in much-disturbed regions, where steep density-gradients occur.

In a paper on "Regular Consequences of Irregular Refraction in the Sun"[2] I attempted to obtain a general idea of the optical effect which local condensations and rarefactions in the solar atmosphere must produce, if only the incurvation of rays is taken into account.[3] The result was as follows.

Let us first consider light for which the refracting power of the solar atmosphere ($n-1=R\Delta$) has a certain *positive* value. Somewhere on the central part of the disk we imagine in the gaseous envelope a region of any shape, only satisfying the condition that, from the outline inward, the density of the gases either diminishes or

[1] By R-light and V-light will be denoted waves on the red and violet sides of absorption lines within the limits where anomalous dispersion is perceptible.

[2] Julius, *Proceedings of the Royal Academy of Amsterdam*, **12**, 266, 1909; *Memorie della Società degli Spettroscopisti italiani*, **38**, 173, 1909; *Physikalische Zeitschrift*, **11**, 56, 1910.

[3] It may be well here to remark, that in the paper referred to, as well as in former publications on anomalous dispersion, I never thought of denying the probable effects of selective radiation, absorption, scattering, radial motion, pressure, radio-activity, magnetism; but because in solar literature full attention is generally paid to most of these subjects, whereas refraction and anomalous dispersion are little noticed, I wished to consider the latter agencies separately, and to inquire which solar phenomena may be produced or influenced by them. The object in view was not a theory of the sun, but a study of the cosmical consequences of anomalous dispersion.

increases continuously, so that the region includes either a minimum or a maximum of density. In both cases the image will show a dark rim. If in these two cases the density-gradients, though opposite in sign, were equal in magnitude, the optical images presented by the rarefaction or the condensation would be almost identical in their principal features. This is due to the fact that the light transmitted by our region comes from a source extending nearly symmetrically round the line of sight. As soon as the latter condition is not fulfilled, if, for instance, some of the rays, before entering our region, had already suffered strong deviation in a neighboring very marked density-gradient, the symmetry of the apparent source of light would be disturbed, and then the aspect of the rarefaction might sensibly differ from that of the condensation of the same shape.

Let us now consider light for which the refracting power of the solar atmosphere is equal in absolute magnitude, but *negative*. Such waves behave in a rarefaction just as the other waves, first considered, would do in the condensation that would be obtained by reversing the gradients. The optical effect is generally the same in its principal features. Consequently, confining our attention to the central parts of the disk, and excluding the much-disturbed regions, we must expect to find only small difference between spectroheliographic images taken with R-light and V-light selected at the proper distances from an absorption line.

Hale and Ellerman admit that the small differences, frequently observed when comparing images given by opposite sides of $H\alpha$, are, perhaps, due to anomalous refraction; I see no reason why the same principle should be inactive in the production of the remaining, almost identical, parts of the images.

As we approach the limb, the conditions of refraction are, however, modified. When seen projected on the disk at a sufficient distance from the center, a region with a minimum and a region with a maximum of density will appear different. With R-light the rarefaction shows *dark* on the side *opposite* the center of the disk, and may be *brighter* than the surroundings on the side *facing* the center, whereas the condensation shows *dark* on the side *facing* the center, and may come out *bright* on the *opposite* side. With V-light these effects are the reverse, rarefaction and condensation optically changing

parts.[1] So we have reason to expect that between spectroheliograms taken with light from the red and violet sides of a line, some systematic differences of detail—increasing as we proceed from the center toward the limb, and relating to distribution of brightness rather than to structure—will be observed.

It will prove necessary, however, to check the latter expectation, because there is a physical law, not hitherto considered in our argument, which tends to efface the differences just mentioned, and to promote similarity of the corresponding R-light and V-light images all over the disk. I mean the fact, discovered by Rayleigh, that the light is *scattered* by the molecules of a transmitting medium.

Effects of scattering on the character of the total radiation transmitted by stellar atmospheres were first considered by Schuster in a most interesting article, "Radiation through a Foggy Atmosphere."[2] It would lie beyond the scope of the present note to discuss the general bearing of the remarkable results, there described, upon conclusions deduced from the anomalous dispersion theory. One point, however, which may prove very important with respect to the explanation of spectroheliograph results, requires our special notice, viz., that scattering is a *selective* process. This peculiarity was alluded to by Schuster on p. 17 of the paper cited, but not further considered there.

Indeed, if we accept Rayleigh's formula, the coefficient of scattering, called s in Schuster's paper, depends not only on the number N of scattering particles per unit volume, and on the wave-length λ of the light under consideration, but also on the index of refraction n of the medium:

$$s=\frac{32\pi^3(n-1)^2}{3N\lambda^4}. \tag{1}$$

The terms "anomalous dispersion" and "anomalous refraction" were until now used indiscriminately. We shall in future distinguish between the two expressions. By anomalous dispersion we denote the general property of matter, that its refracting power $\pm(n-1)$ varies rapidly as we approach an absorption line. This property, of course, subsists even when the density of the medium is perfectly

[1] *Proc. Roy. Acad. Amsterdam*, **12**, 269, 274–276, 1909; *Memorie d. Soc. d. Spettr. ital.*, **38**, 175, 180, 1909; *Physikalische Zeitschrift*, **11**, 58–59, 63–64, 1910.

[2] *Astrophysical Journal*, **21**, 1–22, 1905.

uniform, and the propagation of light rectilinear. Whenever the density is not uniform, it may cause very different deviations of neighboring waves. That effect of anomalous dispersion—which I exclusively studied in former papers on the subject[1]—will be called *anomalous refraction*. Another effect, dependent on the same property, and now considered for the first time, is *anomalous scattering*.

Equation (1) shows that the coefficient of scattering passes through a sharp maximum in the neighborhood of every value of λ which corresponds to an absorption line, because there the factor $(n-1)^2$ increases rapidly as we approach the line from either side. In the nearest vicinity of the absorption lines of a mixture of gases, Rayleigh's formula is perhaps not rigorously applicable, but we may use it as a first approximation.

Even absolutely monochromatic absorption would thus, in an extensive atmosphere, give rise to a line of a certain width. If a group of neighboring waves are absorbed, the width of the resulting dark line will always exceed that of the spectral region of real absorption. Every absorption line of a stellar atmosphere is, therefore, enveloped in what we may call a *dispersion band*, because it depends upon anomalous dispersion. In an atmosphere of perfectly uniform density the dispersion band would be caused by anomalous *scattering* only; but if irregular density-gradients occur, anomalous *refraction* adds to the effect in two ways: (1) by directing back toward the luminous surface some of the strongly refracted rays,[2] and (2) by lengthening the paths along which the beams are subject to loss of intensity by scattering.

These notions may gain clearness if we imagine ourselves to be placed somewhere in the solar atmosphere, looking outward. Then

[1] I am very much indebted to Professor Lorentz of Leiden, who, at my request, was kind enough to subject my preceding work on the consequences of anomalous dispersion to a thorough criticism. According to him the weak side of my conclusions was, that I had not duly noticed the diminution of the light by scattering. I intend to discuss this important point more fully on a later occasion. The resulting new aspect of the anomalous dispersion problem will render necessary certain modifications of the theory (e.g., regarding the explanation of prominences), and may thus perhaps serve to reconcile opposite opinions on this matter.

[2] This process was more fully treated in my paper on "Regular Consequences of Irregular Refraction in the Sun," in the chapter "On the Origin of the Fraunhofer Lines," *op. cit.*

a spectroscope, if directed on the "solar sky," would show us the Fraunhofer lines bright on a less luminous ground, not only on account of luminescence or of selective temperature-radiation, but also because the scattering is more intense in the vicinity of absorption lines than in blank parts of the spectrum. The energy which thus returns to the sun by the *scattering* process is wanting in the Fraunhofer spectrum as seen on earth. Besides, the irregular density-gradients of the solar atmosphere would give rise to "mirage" on a large scale, also of a selective character. Distorted images of parts of the brilliant solar surface would appear everywhere in the sky, different in shape and extension for kinds of light that are differently refracted. This is the portion which anomalous *refraction* contributes to the returning energy, and withdraws from the radiation leaving the sun.

Applying our ideas on the combined consequences of anomalous scattering and refraction to the interpretation of spectroheliograph esults, we must remember: (1) that anomalous scattering darkens the solar spectrum almost equally on both sides of a strong absorption line,[1] thus reducing the differences which photographs made with R-light and V-light at equal distances from the same line would have shown, if anomalous refraction were the only agent; (2) that the width of a Fraunhofer line would be a minimum at points of the sun's image corresponding to regions of uniform density and composition in the solar atmosphere, because there anomalous scattering would be the only cause of the dispersion band; (3) that the same line will be wider, and, in general, darker in the spectrum of regions where irregular gradients disturb the rectilinear propagation of the light. (In this way we explain the varying width of $H\gamma$ as shown in Fig. 2 of Plate I, *Proceedings of the Royal Society*, **83,** 189, 1910. If, therefore, the camera-slit of the spectroheliograph is set, for instance, between the center and the edge of $H\alpha$, but nearer to the edge, the dark flocculi indicate regions where density-gradients with large components perpendicular to the line of sight are in

[1] It will be mentioned farther on, that especially the weaker Fraunhofer lines are asymmetrical by anomalous dispersion. So long as spectroheliograms are made only with light from the domain of strong lines, we may, in interpreting them, neglect that systematic asymmetry.

evidence. Almost the same structure must be revealed if the camera-slit is set on $H\beta$ or $H\gamma$, provided the distance from the center of these lines be taken smaller than with $H\alpha$, in order to catch waves that are refracted to the same degree as those in the former case. This effect was predicted in my paper in the *Astrophysical Journal*, **28**, on p. 369, and afterward found confirmed by Hale and Ellerman.)[1] (4) that gradients of exceptional magnitude and extension may produce marked irregularities in the distribution of the light within the range of a dispersion band; (5) that the composition of the solar atmosphere very probably varies with the level, but that convection currents tend to efface local differences of composition and temperature.

If these statements are kept in mind, it will be found possible to explain, on the basis of anomalous dispersion, at least as many particulars of the spectroheliograms as were explained by Hale and Ellerman on the basis of their temperature and absorption hypothesis. We will not, on this ocasion, enter into a comparison of the advantages of both points of view, our present aim being only to prevent a premature criticism of either of them.

With a similar object in view we shall now consider another important solar phenomenon—systematic displacements of Fraunhofer lines—which also was explained according to two entirely different theories.

I showed elsewhere[2] that anomalous refraction by irregular density-gradients causes the Fraunhofer lines to be asymmetrical, the narrower ones generally to a higher degree than the wider ones, thus producing an apparent displacement of the lines toward the red. The displacements must increase when passing from the center of the disk to the limb. These effects depend upon the rule that the refracting power of the mixture of gases constituting the solar atmosphere is on the average greater on the red side of an absorption line than on the violet side. Anomalous scattering also being determined by the values of the refracting power on both sides of the absorption lines, it co-operates in producing those systematic displacements.

[1] The optical effect produced by the systematized density-gradients near solar vortices requires special treatment.

[2] Cf. the paper on "Regular Consequences," etc., referred to above.

From a recent remarkable investigation of the displacements of the spectrum lines at the sun's limb, by W. S. Adams,[1] it appears that out of a total of 470 lines only one or two are shifted unmistakably toward the violet; the other lines all show displacements to the red, ranging from 0.000 to 0.014 Ångström. The various characteristics of the list of these lines will have to be studied in detail from the point of view of anomalous dispersion. I must defer that inquiry to a later date, and now confine myself to a few remarks on prominent statements made in Adams' paper.

Adams concludes that pressure is the effective agent in producing the displacements observed. He evidently paid very little attention to the possibility of explaining these phenomena by anomalous dispersion, for although he refers to the explanation which I recently published in the *Memorie della Società degli Spettroscopisti italiani*, and rejects it, the clue of my argument escaped his notice. Indeed, he writes:

> According to his [Julius'] point of view the photospheric light is anomalously refracted in the vicinity of the absorption lines produced by the metallic vapors, and, since in general the density-gradient decreases outward, the widening will be upon the red side of the lines producing the observed displacements. The fact that the sodium lines D_1 and D_2 are not displaced, although they show the largest amount of anomalous dispersion of any which have been investigated for this effect, is rather strongly opposed to this view.

In the first place, I do not quite understand why the decrease of the density-*gradient* should be material to the case. This, however, may be a lapse; probably the author intended to say: "since in general the density decreases outward." But then the inference expressed in the sentence as a whole is erroneous. A little reflection will easily show that near the limb the regular radial density-gradient assists R-light and hinders V-light in curving from the photosphere toward the observer. The result would be an apparent displacement of the dark line to the *violet*, not to the red. The radial gradient, therefore, if it is of any importance in this matter, counteracts the effective agent which produces the observed shifts toward the red.

The principal point overlooked by Adams is that, according to my explanation, the effective agent in producing the phenomenon is

[1] *Contributions from the Mount Wilson Solar Observatory*, No. 43; *Astrophysical Journal*, **31**, 30–61, 1910.

the general asymmetry of the dispersion bands enveloping the absorption lines. It does not depend upon the incurvation which rays undergo in the regular radial density-gradient of the solar atmosphere, but is caused by anomalous scattering, and refraction in irregular gradients, combined with the fact that the refracting power of the mixture of gases is on the average greater for R-light than for V-light.

If we keep this in mind, we shall have a useful basis for investigating the relationship between anomalous dispersion and the results of Adams' measurements. That a simple comparison of Geisler's observations on anomalous dispersion of metallic vapors in the arc with displacements at the limb—as given by Adams on p. 28 (*op. cit.*)—could not possibly serve the purpose of finding such a relationship, is evident; for the amount of that part of the displacement which is due to anomalous dispersion is determined by the degree of asymmetry of the Fraunhofer line under consideration; and this asymmetry is not a mere property of the corresponding element itself, revealable in laboratory experiments, but depends upon the concentration with which that element is represented in the solar atmosphere. No shade of proportionality between the results of those two investigations could be expected. So it is not at all opposed to our view that the winged lines of sodium and calcium are little, or not at all, displaced at the limb, although they show strong anomalous dispersion. On the contrary, that result might have been foreseen; for if the wide wings are really owing to that cause, the wave-length corresponding to the zero value of the refracting power of the mixture, which always lies on the violet side of a Fraunhofer line, must be at a rather great distance from the absorbed waves,[1] thus making the asymmetry of the dispersion band imperceptible. The central part of the line, the true absorption line, cannot be displaced by anomalous dispersion.

A peculiar feature of our explanation is that both very strong and very weak anomalous dispersion make the displacements small, whereas intermediate values give larger displacements. Indeed, with decreasing width of the dispersion band, its asymmetry increases; but the resulting apparent displacement can never surpass half the width of the line. (Whenever greater shifts are observed, pressure, or magnetism, or Doppler effect certainly come into play.)

[1] Cf. Fig. 8, *Physikalische Zeitschrift*, **11**, 68, 1910.

The largest displacements observed by Adams occur with many lines of iron and nickel. From the point of view of our hypothesis this means that near these lines the amount of anomalous dispersion of the mixture is most suitable for producing the phenomenon, neither too great, nor too small. Considerably smaller are the displacements for titanium, vanadium, and scandium—perhaps because these elements are less in evidence in the mixture of gases. That those iron lines which are most strengthened at the limb show smaller displacements than the average iron lines, also perfectly fits our point of view, for their asymmetry must be less conspicuous on account of their greater width. That the lines of the elements of very high atomic weight, such as lanthanum and cerium, show very small displacements is easily accounted for if we assume their vapors to be extremely rare in the solar atmosphere. This explanation is certainly not less simple than the one proposed by Adams on pp. 17 and 18 of his paper,[1] where he has to find a way out of the discrepancy to which in that case the pressure hypothesis appears to lead.

Various other characteristics of Adams' interesting list of displacements (e.g., the special behavior of the enhanced lines as a class) will be discussed on a later occasion, together with his equally valuable observations of the spectrum of sun-spots.

UTRECHT
April 1910

[1] *Contributions from the Mount Wilson Solar Observatory*, No. 43; *Astrophysical Journal*, **31**, 46–47, 1910.

CORRECTIONS TO RADIAL VELOCITIES OF CERTAIN STARS OF THE *ORION* TYPE

By EDWIN B. FROST

In our paper[1] entitled "Radial Velocities of Twenty Stars Having Spectra of the *Orion* Type," Mr. Adams and I called attention, on p. 15, to the inaccurate wave-lengths of the three silicon lines at $\lambda\lambda$ 4553, 4568, and 4575, which we were obliged to employ, depending upon the approximate values given by Exner and Haschek for the spark spectrum.

I subsequently redetermined[2] the wave-lengths of these lines from plates made by Mr. Julius A. Brown with our 10½-foot concave grating, using the spark between silicon electrodes. I pointed out that the considerable departures we thus found from the values of Exner and Haschek would very appreciably affect the radial velocities of the *Orion*-type stars previously published from measures on Bruce spectrograms (p. 160). The accordance of the velocities from the separate silicon lines was not particularly increased by the use of the new wave-lengths, but the agreement of the measures of these lines for different grating plates indicated an accuracy within a few hundredths of an Ångström unit. Using the wave-lengths as 4552.636 4567.897, and 4574.791, the corrections to plates reduced with Exner and Haschek's values, expressed in velocities, are respectively 7.51 km, 3.48 km, and 7.14 km, to be applied positively. In the spectra of seven of the twenty stars, the silicon lines were not measured, namely, *ε Cassiopeiae, ζ Orionis, η Leonis, γ Corvi, τ Herculis, ζ Draconis, ε Delphini.* Of these, *γ Corvi* was found to be a spectroscopic binary by Campbell and Curtis. The following table gives the corrections to the radial velocities, and the corrected radial velocities for the eight of the remaining stars which have not thus far been proven to be spectroscopic binaries. For convenience I have added the number of plates used and the epoch. For the five remaining stars having variable radial velocities, the values for the separate plates are given. The variation of *β Orionis* was established by Plaskett, of *γ* and *ε Orionis* by the writer, of *β Canis Majoris* and *η Lyrae* by Albrecht.

[1] *Publications of the Yerkes Observatory*, **2**, 1903.

[2] *Astrophysical Journal*, **22**, 157, 1905.

Star	No. of Plates	Correction	Corrected Radial Velocity	Epoch
γ Pegasi	8	+1.6 km	+ 7.0 km	1902.06
ζ Cassiopeiae	4	+1.6	+ 4.5	1902.10
ζ Persei	5	+1.9	+24.0	1901.95
κ Orionis	7	+1.6	+18.7	1901.88
ε Canis Majoris	3	+1.9	+29.1	1902.61
ι Herculis	4	+1.1	−15.3	1901.92
67 Ophiuchi	3	+1.3	− 1.8	1902.47
102 Herculis	4	+1.9	− 8.8	1902.62

In the case of the following spectroscopic binaries the values are given only for plates where the use of the silicon lines makes corrections necessary.

Star	Plate	Date	G.M.T	Correction	Corrected Radial Velocity
β Orionis	A 262	1901 Oct. 3	$19^h 26^m$	+0.7 km	+23.5 km
	B 207	Oct. 18	20 7	+0.7	+20.7*
γ Orionis	A 224	1901 Sept. 11	21 32	+1.6	+17.0
	A 258	Oct. 2	21 34	+0.4	+16.2
	B 221	Nov. 8	20 38	+1.5	+20.1*
	B 253	Nov. 27	21 55	+1.3	+22.5
	B 262	Dec. 31	15 38	+1.2	+19.2*
	B 299	1902 Mar. 13	15 53	+0.9	+16.7
	B 317	April 9	15 4	+0.7	+19.4*
ε Orionis	A 208	1901 Sept. 4	22 10	+1.5	+29.0
	B 228	Nov. 13	19 50	+0.7	+27.0*
	B 298	1902 Mar. 13	15 11	+0.8	+26.9*
	B 316	April 9	14 21	+0.3	+27.4*
β Canis Majoris	A 287	1901 Oct. 31	21 35	+2.2	+34.5*
	A 293	Nov. 1	21 26	+1.9	+36.7
	B 215	Nov. 7	21 0	+1.7	+33.4*
η Lyrae	B 409	1902 Sept. 13	17 57	+0.6	−10.0
	B 422	Oct. 15	14 12	+0.4	− 5.1*
	B 427	Oct. 16	15 32	+0.7	− 7.8*

*Mean of the measures by Frost and by Adams.

For the sake of uniformity with the earlier publication, the velocities are given to the tenth of the kilometer, but I do not attach any weight to the decimal.

Finality in respect to radial velocities is hardly to be obtained: with improvements in the accuracy of the stellar wave-lengths, the values published are subject to alteration. In measures of stars of the *Orion* type much dependence is necessarily placed upon the double helium line at λ 4472. We have here always employed the blended value 4471.676 resulting from the assignment of weights

6 and 1 to the two components, according to the estimates by Runge and Paschen. Variation in relative intensity of these components in different stars, or an uncertainty in the adopted blend, tends to introduce a systematic error in the radial velocity. Of course this may be adjusted by making the residuals of all lines used for a star add up zero, as is done by some observers. There are seldom lines enough, however, to furnish a thorough balancing of errors. This line is merely cited as a further illustration[1] of inherent uncertainties which make decimals of a kilometer illusory for most stars.

The data establishing the variable velocity of γ *Orionis* have not yet been published, and may be given here. I was recently led to take and measure additional low-dispersion plates of this star from the evidence of double lines I found on a casual one-prism plate.

γ *Orionis* ($\alpha = 5^h 20^m$; $\delta = +6° 16'$; Mag. = 1.9)

Plate	Date	G.M.T.	Taken by	No. Lines	Velocity	Quality
IB2282	1910 Feb. 18	$13^h 6^m$	F.	8	+11 km	v. g.
2284	Feb. 21	12 50	L.	5	+ 8	weak
2285	Feb. 22	13 12	F.	8	+12	g.
2293	Feb. 28	14 22	F., L.	8	+26	v. g.
2295	Mar. 7	12 44	L.	8	+ 1	fair
2303	Mar. 14	13 2	L.	6	+ 6	too strong

F. = Frost; L. = Lee; v. = very; g. = good.

On plate 2282 a faint component to λ 4472 was measured, which yielded a velocity of -108 km. Faint components were suspected in other instances.

It is our experience that low dispersion is more favorable than high dispersion for the detection of faint components of diffuse lines, and one-prism plates of some of these stars originally observed with three prisms have shown duplicities where we should not have been justified previously in suspecting them.

A re-examination of our three-prism plates of γ *Orionis* does not disclose any certain doubling of the lines.

The above data suggest that the period of γ *Orionis* is not very short. The only other published observations of the radial velocity of γ *Orionis* known to me are the measures of three plates at Potsdam, as follows, the values being the means of measures by Vogel and by Scheiner:

1888 Dec. 7, +9 km; 1891 Feb. 1, +8 km; 1891 Feb. 4, +13 km.

YERKES OBSERVATORY
April 26, 1910

[1] *Astrophysical Journal*, **31**, 377, 1910.

THE CORRESPONDENCE BETWEEN ZEEMAN EFFECT AND PRESSURE DISPLACEMENT FOR THE SPECTRA OF IRON, CHROMIUM, AND TITANIUM[1]

By ARTHUR S. KING

The following is an attempt to present such evidence as is available concerning the connection between two phenomena which seem on certain theoretical grounds probably to be related, but for which a quantitative comparison has been almost entirely lacking. The material is still insufficient to make more than a beginning of the study of the relation from the quantitative side. It is hoped, however, that the accumulation of more data in this laboratory and elsewhere may soon add to the experimental evidence on the subject.

The view that there is a direct connection between the Zeeman effect and the pressure displacement of spectrum lines has been strongly advocated by Humphreys in a series of papers[2] which have been summarized[3] by him, together with all other pressure investigations up to the year 1908. Humphreys' hypothesis, briefly stated, is that the part of the atom to which the light impulse is due is a ring of electrons, rotating with a period of the order of the light vibration. Each of the electron rings will then set up a magnetic field of its own. The luminous gas will be in a condition of minimum potential energy when the planes of the rings are parallel and the electrons rotating in the same direction. We must, however, in view of the Zeeman effect, consider that different rings may rotate in opposite directions, and assume merely that the regular condition is a rotation of the electrons in orbits approximately circular, with a tendency for the planes of these to become parallel. The effect of pressure in the surrounding medium will be to bring the rings closer together, thereby altering their mutual induction. If two rings rotating in the same direction

[1] *Contributions from the Mount Wilson Solar Observatory*, No. 46.

[2] *Astrophysical Journal*, **23**, 233, 1906; **26**, 18, 297, 1907; **27**, 194, 1908.

[3] *Jahrbuch der Radioaktivität und Elektronik*, **5**, 324, 1908.

are made to approach, the current in each ring will decrease, which means a retardation of the rotating electrons and an increase of period in the corresponding light vibration, resulting in a shift of the spectrum lines toward the red.

If rings of opposite rotation are forced closer together, their motion will be accelerated, resulting in a shift of the spectrum lines to the violet. Assuming that both directions of rotation are present for electrons producing each spectrum line, the general result will be a widening of all lines as the pressure increases, with a prevailing shift of the maximum of each line toward the red. This last is due to the fact that the condensing action of the pressure on rings rotating in the same direction is assisted by the effort of these rings to get into the strongest part of their mutual field; while for oppositely rotating rings the approach is opposed by the magnetic action, so that on the whole the retardation of the period for a given line is greater than the acceleration, and the line, while being widened toward both red and violet, has its maximum intensity moved toward the red.

Another theory is worked out by Richardson[1] which opposes the connection of pressure displacement with Zeeman effect. Instead of basing his reasoning on magnetic perturbations, Richardson considers the electron as an oscillator which sets up an alternating electrostatic field in its neighborhood. This field would produce forced vibrations in the electrons belonging to neighboring atoms, an effect increased by pressure in the medium. The electric field produced by the forced vibrations would then react on that of the radiating electrons. The mathematical development gives a change of wave-length proportional to the pressure and toward the red. Worked out numerically with the available data, the electrostatic resonance theory requires values for the pressure displacement many times greater than those observed experimentally. A modified conception of the equilibrium conditions might account for this discrepancy.

Richardson objects to Humphreys' theory largely on the ground that the magnetic disturbances of period would be far too small to account for the observed displacements of lines unless the magnetic field for any atom is greater than that corresponding to saturated iron, which Richardson holds to be an upper limit. This is replied

[1] *Philosophical Magazine* (6), **14**, 557, 1907.

to by Humphreys in a later paper[1] in which he questions the right to base the possible magnetic intensity of iron atoms upon the properties of iron in large masses, since the permeability and saturation point depend upon many factors of composition and physical condition. Going farther, Humphreys considers an ideal electron ring and deduces an expression for the change of rotation frequency brought about by an external magnetic field H, such as that due to a neighboring electron ring. This is found to give an expression for the change of wave-length $\Delta\lambda$ in the ether vibrations of original wave-length λ which reduces to $\Delta\lambda/H\lambda^2=C$, a constant, which is Preston's law for the Zeeman phenomenon, indicating that the ideal electron ring is very similar in structure to the actual radiating particle. If this similarity is admitted, Humphreys is justified in his next step, which is the substitution of known values in the expression for the change of wave-length of ether vibrations produced by a change in the period of the electron ring. This gives a field intensity for the rotating ring of 45×10^7, which is about ten thousand times that of the strongest fields used in spectroscopic work. The change in mutual induction by pressing together electron rings having fields of this magnitude may be expected to give shifts of spectrum lines of the order of those measured.

A third theory is that presented by Larmor,[2] who treats the electron as a Hertzian doublet in a field of electric force. This field would be altered by any change in the distribution of material particles in the medium such as would result from increased pressure. A molecule approaching a vibrating electron would decrease the rigidity of the ether at that point. A lowering of the ether strain would tend to increase the period of the electron, and it is shown that this might give displacements of the magnitude observed for spectrum lines. A note by Humphreys[3] points out that several consequences of Larmor's theory agree only to a limited degree with observed facts, while the requirement that the shift should decrease in magnitude with the wave-length is contrary to the regular behavior in spectra.

It would seem that the interacting magnetic atoms of Humphreys provide the most plausible theory among those given; but experi-

[1] *Astrophysical Journal*, **27**, 194, 1908.

[2] *Ibid.*, **26**, 120, 1907.

[3] *Ibid.*, **26**, 297, 1907.

mental data have been lacking to show the probability of a connection between the effects of pressure and magnetic field on spectrum lines. Humphreys considers[1] that, in general, lines of large Zeeman separation are strongly displaced by pressure, but admits that there is scanty material on which to base this conclusion. The refusal of banded spectra, notably that of carbon, to show either Zeeman effect or displacement has often been cited as probably resulting from a connection between the two phenomena, and interesting developments on this point have recently been presented. Dufour[2] obtained Zeeman separations for the component lines of the band spectra of the chlorides and fluorides of the alkaline earths, the magnitude of separation being about the same as for line spectra. A short time after, Rossi[3] selected three of these, the fluorides of calcium, strontium, and barium, and obtained distinct pressure-shifts for the bands, the shifts being of the same order as for line spectra. Comparing his results with those of Dufour, Rossi did not find any general relation between the magnitude of the two effects.

A detailed comparison of the two phenomena for line spectra is very desirable and the writer found it possible to make a beginning by comparing a considerable amount of Zeeman material collected in this laboratory for the spectra of iron, titanium, and chromium with the pressure displacements for the same spectra given by Humphreys[4] and Duffield.[5]

The material on the Zeeman side was obtained from an extended investigation of the iron and titanium spectra by the writer through the range from λ 3660 to λ 6700, in which all lines of fair intensity were photographed, the character of the separation studied, and the components measured. The titanium spectrum has been entirely rephotographed under higher dispersion and stronger field since the publication of the writer's former paper on this spectrum.[6] This material for iron and titanium is being prepared for detailed presenta-

[1] *Astrophysical Journal*, **26**, 29, 1907.

[2] *Comptes Rendus*, **146**, 118, 229, 1908.

[3] *Proceedings Royal Society*, **82**, 518, 1909.

[4] *Astrophysical Journal*, **26**, 18, 1907.

[5] *Philosophical Transactions*, A, **208**, 111, 1908.

[6] *Astrophysical Journal*, **30**, 1, 1909.

tion in the *Publications* of this observatory. The measurements for chromium are from plates taken partly by Mr. Babcock and partly by myself. This series is not yet complete, although almost all lines given in the pressure tables were available for comparison.

The pressure measurements by Humphreys, while they cover only a fraction of the lines available on the Zeeman plates, give a sufficient number of the stronger lines of iron, chromium, and titanium as far as λ 5600 to show what may be expected as to general agreement between the two phenomena. The measurements of Duffield cover only a small region of the iron spectrum, but his values have been used to supplement the list of Humphreys and his classification of lines has proved useful in showing the rate of increase of displacement with pressure.

EXPERIMENTAL METHOD

The complete description of apparatus and methods must be left for the detailed publication. It may be said here, however, that the photographs used for these measurements were taken in the third order of a plane Rowland grating mounted in a vertical Littrow spectrograph,[1] the objective having 13 feet (4 m) focal length. The scale of the photographs was about 1.35 Ångström units to the millimeter. The light-source was a transformer spark between pieces of the metal under investigation, these being held between the poles of a large Dubois electromagnet. The light was taken at right angles to the lines of magnetic force and a Nicol prism above the slit used to transmit either the light with vibrations parallel to the magnetic force-lines or that vibrating in a plane at right angles to the force-lines. Several plates were usually taken for the same region of the spectrum, in order that lines of different intensities might be obtained favorable for measurement. The spectrum of the spark without the magnetic field was always taken for comparison outside of the spectrum with the field by moving an occulting plate above the slit.

EXPLANATION OF TABLES I, II, AND III

The wave-lengths given in column one are those of Rowland for the corresponding solar lines. The measurements of Kayser and

[1] Hale, "The Pasadena Laboratory of the Mount Wilson Solar Observatory," *Contributions from the Mount Wilson Solar Observatory*, No. 27; *Astrophysical Journal*, **28**, 244, 1908.

Runge and of Hasselberg were used by Humphreys and Duffield, but there has been as a rule no difficulty in selecting the proper line from the Rowland table. Column two describes the character of the Zeeman separation. This description includes the components given with the Nicol prism in two positions 90° apart and gives the appearance when the spark is observed at right angles to the force-lines without a Nicol. An interrogation point is often used to indicate that the type of separation is somewhat uncertain. When a triplet is thus marked, it usually means that the middle component is slightly widened, so that with a much stronger field it would probably be resolved into two. A questionable quadruplet has the components of its outer pair widened so that such a line may be a sextuplet. If the outer components are so much widened that each is certainly made up of two and possibly more, the line is a doubtful sextuplet, and correspondingly for the other types.

The "Weight" of the measurement for the Zeeman components is given in column three, and indicates the quality of the line as good, fair, or poor, corresponding to weights of 3, 2, and 1, respectively. Lines of weight 3 have sharply defined components, with an error of measurement in the third decimal place. Lines whose components are widened and probably compound or poorly defined for any reason do not admit of such close measurement and are graded 2. Those lines whose components are very diffuse, weak, or disturbed by blends, so that the measurement gives little more than the order of magnitude, are weighted 1.

The letters *n* and *p* (corresponding to *s* and *p* in German publications) are used in the table to denote the components having vibrations in a plane normal to the force-lines and parallel to the force-lines respectively. Column four gives the measured separation in Ångström units of the *n* components, the mean being taken when there are two or more pairs. The values of $\Delta\lambda$ for chromium and titanium may be slightly altered when the final tables are published, as the material for these spectra has not been fully worked over. The separations here given are accurate enough to show the order of magnitude. The behavior of the *p* component, which is often split up, is not given here, as it can scarcely enter into a comparison with pressure displacement, the electron in the latter phenomenon being

assumed to have a circular orbit. The character of the *p* component, as well as all description of the widening of components, is left for the complete Zeeman tables.

The measurements of pressure displacements by Humphreys are given in column five. These are in Ångström units and for a pressure of 42 atmospheres, his other measurements, for 69 and 101 atmospheres, being for only a part of the lines. For the iron spectrum, the displacements of Duffield for 41 atmospheres are given in the next column. Occasionally a line was not obtained by these observers for the given pressures, in which case an approximate value was deduced from the measurement for some other pressure and this noted in the "Remarks" column.

The two columns preceding "Remarks" contain ratios of Zeeman separation to pressure displacement, the one numerical, the other of letters denoting the order of magnitude. In the numerical ratios, the values of Humphreys are used for the sake of uniformity, those of Duffield for an almost equal pressure being taken when a line was not measured by the former. The letters stand for small, medium, and large values of separation and displacement. The limits covered by these classes are as follows:

	Separation	Displacement
S	<0.300	<0.060
M	0.301–400	0.061–100
L	>0.401	>0.101

The reasons for this classification are given in the discussion.

TABLE I

ZEEMAN SEPARATION AND PRESSURE DISPLACEMENT FOR IRON

λ	Character of Separation	Wt.	Separation $H=16,000$	Displacement		Ratio Sep. to Displ.	Classes Sep. and Displ.	Remarks
				42 Atm. (Humphreys)	41 Atm. (Duffield)			
3659.663	triple	2	0.176	0.050		3.54	S:S	
3669.666	triple ?	2	0.176	0.050		3.54	S:S	
3670.240	triple ?	2	0.261	0.047		5.55	S:S	
3676.457	triple	3	0.236	0.050		4.72	S:S	
3677.764	triple	3	0.184	0.052		3.54	S:S	
3680.069	sextuple ?	3	0.308	0.062		4.97	M:M	
3683.229	triple	2	0.480	0.040		12.00	L:S	*n* comps. have inner fringes
3684.258	triple ?	3	0.170	0.053		3.21	S:S	
3687.610	triple	3	0.313	0.090		3.48	M:M	
3689.614	sextuple ?	2	0.373	0.084		4.44	M:M	
3695.194	triple	3	0.261	0.070		3.73	S:M	
3704.603	triple ?	3	0.319	0.046		6.93	M:S	
3705.708	sextuple ?	3	0.294	0.054		5.44	S:S	
3709.389	triple	3	0.312	0.095		3.28	M:M	
3716.054	quadruple ?	1	0.290	0.107		2.71	S:L	
3720.084	triple ?	2	0.268	0.047		5.70	S:S	
3722.729	sextuple	2	0.286	0.050		5.72	S:S	
3724.526	triple	3	0.256	0.054		4.74	S:S	
3727.778	triple	3	0.324	0.100		3.24	M:M	
3733.469	quintuple	3	0.325	0.050		6.50	M:S	3 *n*, 2 *p* comps.
3735.014	triple	2	0.310	0.092		3.37	M:M	
3737.281	triple	2	0.254	0.040		6.35	S:S	
3738.454	triple	3	0.207	0.078		2.65	S:M	
3743.508	octuple	3	0.343	0.100 ?		3.43	M:M	5 *n*, 3 *p* comps. Δλ mean of 2 pairs. [0.155 for 69 atm.
3745.717	triple	2	0.228	0.050		4.56	S:S	
3746.058	unseparated			0.050			O:S	
3748.408	?	2	0.171	0.040		4.28	S:S	*n* comps. have oute fringes
3749.631	triple	2	0.298	0.085		3.51	S:M	

3758.375	triple	3	0.278	0.090		3.09	S:M	
3763.945	triple	3	0.218	0.095		2.29	S:M	
3765.689	triple	3	0.232	0.106		2.18	S:L	
3767.341	unseparated			0.118			O:L	
3788.046	octuple	2	0.347	0.090		3.86	M:M	5 *n*, 3 *p* comps. $\Delta\lambda$ mean of 2 pairs
3795.147	triple	2	0.334	0.093		3.59	M:M	
3798.655	triple	3	0.326	0.085		3.84	M:M	
3799.693	triple	3	0.326	0.075		4.35	M:M	
3805.486	triple ?	3	0.204	0.092		2.22	S:M	
3813.100	triple ?	2	0.236	0.058		4.07	S:S	
3815.987	triple	2	0.264	0.110		2.40	S:L	
3820.586	triple	2	0.282	0.125		2.26	S:L	
3824.591	triple	3	0.345	0.040		8.63	M:S	
3826.027	triple	2	0.274	0.090		3.04	S:M	
3827.980	triple	2	0.256	0.102		2.51	S:L	
3834.364	triple ?	2	0.248	0.110		2.25	S:L	
3840.580	?	2	0.179	0.098		1.83	S:M	*n* comps. have outer fringes
3841.195	triple	2	0.178	0.100		1.78	S:M	
3850.118	unseparated			0.082			O:M	
3856.524	triple	3	0.341	0.038		8.97	M:S	
3860.055	triple	3	0.347	0.042		8.26	M:S	
3865.674	quintuple	3	0.350	0.103		3.40	M:L	3 *n*, 2 *p* comps.
3872.639	sextuple ?	2	0.289	0.108		2.68	S:L	
3878.720	triple	3	0.346	0.044 ?		7.86	M:S	0.066 for 69 atm.
3886.434	triple	3	0.348	0.056		6.21	M:S	
3887.196	sextuple ?	2	0.342	0.073		4.68	M:M	
3888.671	octuple ?	1	0.231	0.089		2.60	S:M	Probably 6 *n*, 2 *p* comps.
3893.542	triple	3	0.269	0.072		3.74	S:M	
3895.803	triple	3	0.347	0.030		1.16	M:S	
3899.850	triple	3	0.358	0.036		9.94	M:S	
3903.090	sextuple ?	2	0.290	0.095		3.05	S:M	
3904.052	triple ?	2	0.233	0.056		4.16	S:S	
3906.628	triple	3	0.352	0.050		7.04	M:S	
3920.410	triple	3	0.363	0.033		1.10	M:S	
3923.054	triple	3	0.356	0.032		1.11	M:S	
3928.075	triple	3	0.344	0.038		9.05	M:S	
3930.450	triple	3	0.354	0.047		7.53	M:S	

TABLE I—*Continued*

λ	Character of Separation	Wt.	Separation $H=16,000$	Displacement: 42 Atm. (Humphreys)	Displacement: 41 Atm. (Duffield)	Ratio Sep. to Displ.	Classes Sep. and Displ.	Remarks
3948.925	triple	3	0.234	0.050		4.68	S:S	
3950.102	triple	3	0.364	0.066		5.52	M:M	
3956.819	triple	2	0.311	0.036		8.64	M:S	
3969.413	triple	2	0.378	0.089		4.25	M:M	
3977.891	triple	3	0.448	0.042		10.67	L:S	
3981.917	sextuple ?	2	0.240	0.060 ?		4.00	S:S	Given by Rowland as *Ti*. 0.150 for 101 atm.
3984.113	triple	3	0.216	0.085		2.54	S:M	
3986.321	triple ?	2	0.196	0.061		3.21	S:M	
3997.547	triple	3	0.270	0.048		5.63	S:S	
3998.205	triple ?	2	0.226	0.066		3.42	S:M	
4005.408	7 or 9 comp.	2	0.399	0.103		3.87	L:L	
4009.864	?	2	0.342	0.040		8.55	M:S	*n* comps. have outer fringes
4014.677	triple	2	0.259	0.050		5.18	S:S	
4017.308	triple ?	2	0.364	0.062		5.87	M:M	
4022.018	triple	2	0.272	0.037		7.35	S:S	
4045.975	triple	2	0.320	0.103	0.082	3.11	M:L	
4063.759	triple	2	0.269	0.107	0.082	2.51	S:L	
4071.908	triple	2	0.170	0.092	0.086	1.85	S:M	
4107.649	triple	3	0.410	0.060		6.83	L:S	
4109.953	sextuple ?	2	0.322	0.062		5.19	M:M	
4118.708	triple	2	0.265	0.085	0.099	3.12	S:M	
4127.767	triple	2	0.214		0.082	2.61	S:M	
4132.235	sextuple ?	2	0.415	0.105	0.108	3.95	L:L	Probably 4 *n*, 3 *p* comp.
4134.840	triple ?	2	0.291	0.055 ?	0.086	5.29	S:S	0.138 for 101 atm. (Humphreys)
4143.572	triple ?	2	0.280		0.095		S:M	0.048 for 21 atm.
4144.038	septuple ?	3	0.393	0.116	0.099	3.97	M:L	Probably 4 *n*, 3 *p* comps.
4154.667	triple	1	0.379		0.086	4.41	M:M	
4156.970	quadruple	2	0.367	0.064 ?	0.065	5.73	M:M	0.096 for 69 atm.
4175.806	triple	2	0.296		0.065	4.55	S:M	
4181.919	triple	3	0.339	0.070 ?		4.84	M:M	0.170 for 101 atm.

4185.058	triple ?	2	0.412	0.040	0.047	10.30	L:S	
4187.204	triple ?	3	0.372		0.190	1.96	M:L	
4187.943	triple ?	3	0.402		0.431	0.93	L:L	
4191.595	9 comps.	2	0.243		0.310	0.78	S:L	Probably 6 *n*, 3 *p* comps.
4195.492	triple ?	2	0.306		Large		M:L	0.250 at 16 atm.
4196.372	triple ?	1	0.359		Large		M:L	0.260 at 16 atm.; faint in spark
4198.494	triple	3	0.383		Large		M:L	0.147 at 15 atm.
4199.267	triple	3	0.276	0.073	0.065	3.78	S:M	
4202.198	sextuple ?	3	0.331	0.071	0.078	4.66	M:M	
4204.101	triple	2	0.368		0.060	6.13	M:S	
4210.494	triple	3	0.806		0.157	5.13	L:L	
4219.516	triple	3	0.284	0.074	0.078	3.84	S:M	
4222.382	triple	3	0.457		0.358	1.28	L:L	
4227.606	triple ?	2	0.337		0.431	0.78	M:L	
4233.772	9 comps. ?	2	0.236	0.240	0.370	0.98	S:L	Probably 6 *n*, 3 *p* comps.
4236.112	triple	3	0.446	0.274	0.405	1.63	L:L	
4245.422	triple	2	0.464	0.060		7.73	L:S	
4250.945	?	2	0.277	0.089	0.082	3.11	S:M	Numerous *n* comps. blurred
4260.640	triple	3	0.442	0.246	0.177	1.80	L:L	
4271.934	triple	2	0.341	0.083	0.069	4.11	M:M	
4282.566	septuple ?	2	0.360	0.043	0.056	8.37	M:S	
4294.301	sextuple ?	2	0.319	0.084	0.086	3.80	M:M	
4299.410	triple	3	0.415		0.313	1.33	L:L	
4308.081	triple	3	0.322	0.090	0.060	3.58	M:M	
4315.262	triple ?	3	0.558	0.036	0.041	15.50	L:S	
4325.939	triple	2	0.266	0.097	0.56	2.74	S:M	Given by Duffield as 4325.19, which is a very [weak *Fe* line
4337.216	sextuple ?	2	0.295	0.090	0.082	3.28	S:M	
4352.908	septuple ?	2	0.438	0.052	0.056	8.42	L:S	
4367.749	triple	2	0.339	0.060		5.65	M:S	
4369.941	triple	3	0.287	0.055	0.060	5.22	S:S	
4376.107	triple	2	0.471	0.039	0.047	12.08	L:S	
4383.720	triple	3	0.332	0.125	0.060	2.66	M:L	
4404.927	triple	3	0.334	0.110	0.056	3.04	M:L	
4407.871	triple	2	0.631	0.180		3.51	L:L	
4408.582	triple ?	2	0.448	0.160		3.05	L:L	
4415.293	triple	2	0.338	0.087	0.078	3.89	M:M	
4422.741	sextuple ?	1	0.310	0.065	0.046	4.77	M:M	

TABLE I—*Continued*

λ	Character of Separation	Wt.	Separation $H=16,000$	Displacement: 42 Atm. (Humphreys)	Displacement: 41 Atm. (Duffield)	Ratio Sep. to Displ.	Classes Sep. and Displ.	Remarks
4427.482	triple	3	0.460	0.055	0.043	8.36	L:S	
4430.785	triple	2	0.761	0.190	0.159	4.01	L:L	
4442.510	quadruple	2	0.510	0.190	0.164	2.68	L:L	
4443.365	triple ?	1	0.193	0.060	0.060	3.22	S:S	
4447.892	quadruple ?	2	0.674	0.180	0.172	3.74	L:L	
4454.552	quadruple	2	0.445	0.080		5.56	L:M	
4459.301	quadruple ?	2	0.464	0.160	0.172	2.90	L:L	
4461.818	triple	3	0.464	0.060	0.039	7.73	L:S	
4466.727	triple	2	0.384	0.056	0.046	6.86	M:S	
4476.185	triple	2	0.323	0.072	0.042	4.49	M:M	
4494.738	triple ?	2	0.353	0.200	0.168	1.77	M:L	
4528.798	triple	2	0.410	0.172	0.172	2.38	L:L	
4531.327	triple ?	2	0.400	0.075	0.078	5.32	L:M	
4548.024	triple	2	0.366	0.097		3.77	M:M	
4592.840	triple ?	2	0.416	0.110		3.78	L:L	Weak in spark
4603.126	triple	2	0.577	0.093		6.20	L:M	
4647.617	triple	2	0.392	0.070		5.60	M:M	
4691.602	triple	2	0.358	0.070		5.11	M:M	
4710.471	triple ?	1	0.242	0.060			S:S	Blend with air line in spark
4736.963	triple	2	0.426	0.085		5.01	L:M	
4787.003	triple	2	0.409	0.076		5.38	L:M	
4789.849	triple	2	0.352	0.080		4.40	M:M	
4859.928	octuple	2	0.564	0.390		1.45	L:L	5 *n*, 3 *p* comps., center comp. strong
4871.512	?	1	0.336	0.420		0.80	M:L	Comps. blurred, probably 7 or more
4878.407	triple	3	1.032	0.400		2.73	L:L	
4919.174	sextuple ?	2	0.591	0.375		1.58	L:L	
5171.778	triple	3	0.521	0.075		6.95	L:M	
5195.113	triple	3	0.457	0.080		5.71	L:M	
5269.723	triple	2	0.501	0.083		6.04	L:M	
5328.236	triple ?	2	0.470	0.100		4.70	L:M	

5371·734	triple ?	2	0.413	0.095		4·35	L:M	
5397·344	quadruple	2	0.630	0.080		7·88	L:M	
5405·989	quadruple ?	2	0.281	0.100		2.81	S:M	
5429·911	sextuple ?	2	0.607	0.085		7·14	L:M	
5434·740	unseparated			0.120			O:L	
5447·130	sextuple ?	2	0.536	0.095		5·64	L:M	
5455·834	quintuple	2	0.680	0.105		6.48	L:L	3 *n*, 2 *p* comps.
5497·735	octuple	2	1.040	0.110		9·45	L:L	5 *n*, 3 *p* comps.; $\Delta\lambda$ mean of 2 pairs
5501·683	sextuple ?	2	1.001	0.095		10·54	L:M	
5507·000	septuple ?	2	1.026	0.120		8·55	L:L	Probably 4 *n*, 3 *p* comps.
5615·877	triple	2	0.586	0.080		7·33	L:M	

TABLE II

ZEEMAN SEPARATION AND PRESSURE DISPLACEMENT FOR CHROMIUM

λ	Character of Separation	Wt.	Separation $H=17,500$	Displacement 42 Atm. (Humphreys)	Ratio Sep. to Displ.	Classes Sep. and Displ.	Remarks
3885.364	triple	3	0.361	0.060	6.02	M:S	
3894.165	triple	3	0.368	0.072	5.11	M:M	
3908.900	triple	3	0.356	0.049	7.27	M:S	
3916.383	triple	3	0.368	0.062	5.94	M:M	
3919.309	triple	3	0.367	0.052	7.06	M:S	
3921.188	triple	3	0.368	0.076	4.84	M:M	
3928.783	triple	3	0.370	0.050	7.40	M:S	
3941.637	triple	3	0.368	0.046	8.00	M:S	
3963.831	triple ?	2	0.283	0.080	3.54	S:M	All Zeeman comps. displ. toward violet
3969.899	sextuple	2	0.543	0.063	8.62	L:M	$\Delta\lambda$ for outer *n* comps., inner very close
3976.839	sextuple ?	2	0.478	0.058	8.24	L:S	All Zeeman comps. displ. toward red
3984.059	triple	3	0.208	0.140	1.49	S:L	All Zeeman comps. displ. toward violet
3990.140	triple ?	2	0.297	0.056	5.30	S:S	All Zeeman comps. displ. toward violet
3991.333	triple ?	2	0.172	0.070	2.46	S:M	All Zeeman comps. displ. toward violet
3992.950	triple	3	0.430	0.066	6.52	L:M	
4012.631	triple	2	0.238	0.080	2.98	S:M	
4039.244	triple	2	0.263	0.067	3.93	S:M	
4048.910	triple ?	2	0.243	0.070	3.47	S:M	
4058.915	triple	2	0.250	0.076	3.29	S:M	
4126.673	tripl	2	0.380	0.040	9.50	M:S	
4254.505	?	1	0.458	0.056	8.18	L:S	Numerous *n* comps.
4274.958	triple ?	2	0.576	0.076	7.58	L:M	
4280.556	triple ?	2	0.248	0.061	4.07	S:M	
4289.885	?	1	0.544	0.087	6.25	L:M	Many comps.
4295.914	triple	2	0.513	0.056	9.16	L:S	
4297.908	triple ?	1	0.217	0.061	3.56	S:M	
4301.332	triple	1	0.406	0.052	7.81	L:S	Faint in spark
4323.772	triple	2	0.376	0.050	7.52	M:S	
4344.670	triple	2	0.364	0.057	6.39	M:S	

4351.930	triple	3	0.389	0.065	5.98	M:M	
4359.784	octuple ?	2	0.364	0.066	5.52	M:M	Probably 6 *n*, 2 *p* comps.
4363.267	triple ?	3	0.253	0.064	3.95	S:M	
4371.442	sextuple ?	2	0.422	0.060	7.03	L:S	
4497.023	?	2	0.455	0.040	11.38	L:S	*n* comps. have outer fringes
4526.632	triple	3	0.443	0.080	5.54	L:M	
4535.879	quintuple	2	0.737	0.075	9.83	L:M	3 *n*, 2 *p* comps., very unsymmetrical
4546.129	triple	3	0.646	0.060	10.77	L:S	
4580.228	?	2	0.564	0.040	14.10	L:S	*n* comps. have outer fringes.
4600.932	quadruple ?	2	0.549	0.085	6.46	L:M	
4613.544	triple	3	0.880	0.050	17.60	L:S	
4616.305	sextuple ?	2	0.572	0.053	10.79	L:S	
4626.358	sextuple	3	0.698	0.056	12.46	L:S	$\Delta\lambda$ mean of 2 pairs
4646.347	quadruple ?	2	0.450	0.065	6.92	L:M	Very strong in spark
4651.461	9 comps.	2	0.536	0.095	5.64	L:M	6 *n* comps. almost equally spaced, 3 *p* comps., $\Delta\lambda$ is mean of *n* comps.
4652.343	?	2	0.398	0.058	6.86	M:S	*n* comps. fringed. Probably 3 *p* comps.
4680.658	triple ?	1	0.232	0.056	4.14	S:S	Faint in spark
4729.864	triple	1	0.390	0.129	3.02	M:L	Focus poor for this region of plate
4730.897	triple	1	0.402	0.101	3.98	L:L	Focus poor for this region of plate
4756.300	triple	1	0.397	0.146	2.72	M:L	Focus poor for this region of plate
5204.680	9 comps.	2	0.553	0.164	3.37	L:L	$\Delta\lambda$ is mean of 3 pairs
5206.215	sextuple	2	0.701	0.156	4.49	L:L	$\Delta\lambda$ is mean of 2 pairs
5208.596	many comps.	2	0.524	0.092	5.70	L:M	
5247.737	triple	3	0.960	0.132	7.27	L:L	
5348.511	quadruple	2	0.622	0.196	3.17	L:L	

TABLE III

ZEEMAN SEPARATION AND PRESSURE DISPLACEMENT FOR TITANIUM

λ	Character of Sep.	Wt.	Separation $H=17,500$	Displacement 42 Atm. (Humphreys)	Ratio Sep. to Displ.	Classes Sep. and Displ.	Remarks
3685.339	triple	2	0.232	0.012	1.93	S:S	Very strong
3904.926	triple	3	0.238	0.073	3.26	S:M	
3948.818	triple	2	0.191	0.045	4.24	S:S	
3956.476	triple ?	2	0.224	0.030	7.47	S:S	
3958.355	triple	3	0.282	0.045	6.27	S:S	
3981.917	quadruple	2	0.201	0.056	3.59	S:S	
3989.912	triple	3	0.286	0.049	5.84	S:S	
3998.790	triple	3	0.325	0.047	6.91	M:S	
4009.079	triple ?	2	0.359	0.055	6.53	M:S	
4286.168	sextuple	2	0.410	0.103	3.98	L:L	
4287.566	sextuple	2	0.436	0.087	5.01	L:M	
4291.114	quintuple	2	0.450	0.115	3.91	L:L	3 *n*, 2 *p* comps.
4295.914	unseparated			0.100		O:M	
4300.732	triple	2	0.367	0.104	3.53	M:L	
4301.158	triple	2	0.299	0.110	2.72	M:L	
4306.078	sextuple	2	0.366	0.104	3.52	M:L	
4318.830	triple	3	0.340	0.042	8.10	M:S	*Ca?* λ given by Hasselberg; Δλ for 4318.817
4427.266	triple	3	0.316	0.024 ?	1.32	M:S	0.037 for 69 atm.
4533.419	triple	3	0.462	0.176	2.63	L:L	
4534.953	triple	2	0.457	0.124	3.77	L:L	
4544.864	octuple	2	0.513	0.080	6.41	L:M	5 *n*, 3 *p* comps., Δλ mean of *n* separations
4682.088	triple	3	0.395	0.077	5.13	M:M	
4691.523	triple	2	0.422	0.080	5.28	L:M	
4758.308	triple	3	0.376	0.067	5.61	M:M	
4759.463	triple	3	0.435	0.092	4.73	L:M	
4841.074	triple	3	0.390	0.029	13.45	M:S	
4981.912	triple	2	0.487	0.077	6.32	L:M	Very strong
4991.247	triple	2	0.467	0.135	3.46	L:L	Very strong
4999.689	triple	2	0.418	0.120	3.48	L:L	Very strong
5007.398	triple	2	0.346	0.150 ?	2.31	M:L	0.225 at 69 atm.
5013.479	triple	3	0.469	0.056	8.38	L:S	

PROBABLE ACCURACY OF MEASURED SEPARATIONS AND DISPLACEMENTS

While the experience of the writer has been confined to the Zeeman side of the material used in this paper, a few general considerations familiar to everyone accustomed to handling spectrum photographs give a basis of judging the degree of precision to be expected in the measurement of both separations and displacements. On a given plate only a small percentage of the lines are as a rule altogether satisfactory. For measurements of high accuracy, the exposure time, circuit conditions governing the sharpness of lines, complex separations on Zeeman plates, the presence or absence of reversals, and unsymmetrical broadening in pressure photographs are a few of the factors which require a large number of plates, each adapted to the treatment of a certain class of lines, unless there are to be wide differences in the measurement of lines on different plates and by different observers even when the experimental conditions have been made as nearly alike as possible. On this account, Zeeman measurements by different workers do not agree well except for those lines whose components are clearly resolved and sharply defined. In the same way, this difference in quality of lines explains why observers using a series of increasing pressures do not obtain closely proportional displacements for the majority of lines, and also why the measurements of Humphreys and Duffield in columns five and six, of Table I, made for nearly equal pressures, occasionally show large disagreement. As the weight of the measurement is not given for the pressure displacements, a comparison of numerical values for individual lines must take account chiefly of the order of magnitude of the quantities used, since the probable error of one or both of them is frequently large.

DISCUSSION

The question as to whether there is a close proportionality between the numerical values of Zeeman separation and pressure-shift is decided in a definite manner by the column in Tables I, II, and III giving the numerical ratio of separation to displacement. The separations for each spectrum are taken for a constant field and the displacements for a constant pressure. What has been said concerning probable errors in measurement can explain only in a very small

degree the large differences in these ratios. The lack of a constant ratio is very evident. For iron the ratio-values run from 0.78 to 15.5, with such a distribution between these limits that any range which might reasonably be assumed as due to poor measurements covers but a fraction of the lines. Thus in Table I ratios ranging from 3.00 to 6.00 take in 84 out of 166 lines, or 52 per cent. The range from 3.00 to 5.00 includes 64 lines, or 38 per cent. The ratios for titanium and chromium in Tables II and III show divergences of the same order. The lack of constancy in the ratio being apparent, the question arises as to whether there is any real connection between separation and displacement. A broad classification of the values in order of magnitude may be of service in this connection. For this purpose the separation and displacement values are classified as small, medium, and large, the range for each class being as given in the explanation of the tables. The ratios showing the comparative magnitudes of separation and displacement for each line are given in the tables in the column preceding the remarks. The following summary of the data, taking the spectra in turn, will show to what extent a general agreement exists between the Zeeman and pressure phenomena.

1. *Iron.*—The ratios of classes from Table I enable us to place the 173 iron lines in three main groups. Group 1 consists of the ratios S:S, M:M, and L:L, and shows that the separation and displacement for the corresponding lines are relatively of the same order. Group 2 contains those lines for which separation and displacement are not in the same, but in adjacent, classes; while for Group 3 the separation and displacement are of very different magnitudes, one small and the other large. The four lines which show no Zeeman effect but distinct pressure displacement are also in Group 3, the letter O being associated with S, M, or L according to the magnitude of the displacement. The number of lines in these groups is given in Table IV.

From Table IV we see that 44 per cent of the iron lines are in good agreement as to order of magnitude, 42 per cent show a probable discordance, while 14 per cent strongly contradict the hypothesis of equality of relative magnitude. This shows clearly that the two phenomena are not very closely related as regards size of one increas-

ing with size of the other. The large number of lines in Group 2 renders any positive conclusion difficult on account of the possible influence of errors of measurement. Trials with other limits for the small, medium, and large classes have shown that the group percentages are not materially altered, as this results in a transfer back and forth of lines near the limits chosen. An attempt to reduce Group 2

TABLE IV

SUMMARY OF CLASSES—IRON

Group	1			2				3					
Ratio of magnitude....	S:S	M:M	L:L	S:M	M:S	M:L	L:M	S:L	L:S	O:S	O:M	O:L
Number of lines.......	24	30	22	26	20	12	15	10	10	1	1	2
Group total...........	76			73				24					
Group percentage......	44			42				14					

was made by taking all those lines which had one or both values so near the limit of the class that the error of measurement, if in the favorable direction, might have put the two values into the same class and so brought the line into Group 1. Lines of complex Zeeman separation were also treated in this way. Thirty-five iron lines were thus selected, which when added to Group 1 as given in Table IV raised its total to 64 per cent of the whole. This number, then, may be in fair agreement as to order of magnitude, while the remaining 36 per cent are divergent beyond the errors of measurement and in some distances widely different. This last device is of course not a fair treatment of the data, since the error of measurement is as likely to move the values wider apart as closer together, and if the same treatment had been applied to the lines of Group 1, some of them would have moved into Group 2. However, giving the agreement hypothesis the benefit of the doubt, the proportions of 64 and 36 per cent appear to be the most favorable that can be gotten out of the list of iron lines.

The lines in Group 3 deserve special notice. Besides twenty lines for which either separation or displacement is small and the other large, we have the four lines λ 3746.058, λ 3767.341, λ 3850.118, λ 5434.740 which show no Zeeman separation, while one has small,

one medium, and two of them large pressure displacements. None of these lines shows any perceptible widening under a field of 20,000 gausses. They appear to be a striking example of ability to respond to one displacing agency and not to the other. There is a bare possibility that these lines may be of complex structure, with a strong central component and very weak side components which have not been observed. This is very improbable, however, as the lines have been obtained on very strong photographs, and all lines which approach this type, having a strong central *n* component and weaker ones at the sides, have their *p* component also divided into two or more parts.

A comparison of averages for large groups of lines is given in Tables V and VI. The method in forming Table V was to make a list of all pressure displacements classified as small, place opposite them the Zeeman separations for the same lines, and take the mean of each list for comparison of the magnitude of the two effects for all lines of small pressure displacement. Means were formed in the same way for lines of medium and of large displacement. The ratios of mean separation to mean displacement can be then compared. In obtaining the results for each class, means were formed for the lines in three groups according to wave-length. The whole table thus gives a comparison of the means for the several groups, and also an indication as to how the means for both separation and displacement change with the wave-length.

Table VI was made in the same way as Table V, except that here the class of Zeeman separation, small, medium, or large, was taken as the basis, and the corresponding pressure displacements used for a comparison of means.

In Table V the ratios of classes for the three magnitudes of displacement are M:S, M:M, and L:L. Table VI gives for the three magnitudes of separation the ratios S:M, M:M, L:L. There is thus good agreement as to magnitudes except for the first class in each table. Two-thirds of the lines for this class come from the region 3600–4000 and there is a sufficient scattering of high values for both separation and displacement to put the means into different classes when formed in this way. The behavior of the ratios of weighted means in the two tables is interesting, Those in Table V

decrease very nearly in the ratio 3:2:1 for the three classes, showing that the displacements increase in size much faster than the separations. This is not shown so well in Table VI, where the same material is used. While the separation means increase nearly as 4:6:9 the displacement means change slowly. It is probable that the change as shown in Table V is a real one and that it is obscured in Table VI

TABLE V

MEANS OF SEPARATION AND DISPLACEMENT CLASSIFIED ACCORDING TO AMOUNT OF DISPLACEMENT

	Range of λ	No. Lines	Means		Ratio Sep./Displ.
			Sep.	Displ.	
Displacement: small	3660–4000	35	0.293	0.046	6.37
	4000–4500	18	0.376	0.051	7.38
	4500–5600	1	0.242	0.060	4.33
Total no. lines and weighted means		54	0.320	0.048	6.68
Displacement: medium	3660–4000	30	0.279	0.084	3.32
	4000–4500	22	0.310	0.080	3.89
	4500–5600	19	0.489	0.085	5.75
Total no. lines and weighted means		71	0.345	0.083	4.16
Displacement: large	3660–4000	8	0.276	0.109	2.54
	4000–4500	24	0.437	0.207	2.11
	4500–5600	9	0.684	0.245	2.80
Total no. lines and weighted means		41	0.460	0.196	2.34

by the large difference in range of values of separations and displacements. The limits of this range are in the ratio of about 1 to 3 for the separations (omitting a few extreme values) and about 1 to 10 for the displacements. Thus, in Table V, when the displacements are grouped so as to increase in magnitude, there is a much smaller variation among corresponding values of separation than we have among the displacement values when the separations are graded as in Table VI. The widely divergent values of displacement scattered through Table VI would thus act to make the ratios of means more or less discordant.

In Tables V and VI the division into regions of wave-lengths shows the distribution of magnitudes in these regions. Following down the column headed "No. Lines" in each table, we see that the region of shortest wave-length gives the largest number of small values for both separation and displacement. For the medium and large values in each table, the proportion of lines increases in the region of

TABLE VI

MEANS OF SEPARATION AND DISPLACEMENT CLASSIFIED ACCORDING TO AMOUNT OF SEPARATION

	Range of λ	No. Lines	Means		Ratio Sep./Displ.
			Sep.	Displ.	
Separation: small	3660–4000	41	0.239	0.073	3.26
	4000–4500	17	0.257	0.099	2.59
	4500–5600	2	0.261	0.080	3.26
Total no. lines and weighted means		60	0.245	0.081	3.02
Separation: medium	3660–4000	30	0.340	0.064	5.31
	4000–4500	25	0.348	0.101	3.44
	4500–5600	6	0.317	0.135	2.35
Total no. lines and weighted means		61	0.346	0.086	4.02
Separation: large	3660–4000	2	0.464	0.041	11.32
	4000–4500	22	0.501	0.156	3.21
	4500–5600	21	0.617	0.138	4.47
Total no. lines and weighted means		45	0.554	0.143	3.88

greater wave-length, this being very decided for the "large" group. Thus there is a clear increase in magnitude of both separation and displacement as the wave-length increases. The lines here compared seem to be representative of the spectrum, as the same relation holds in the complete Zeeman tables, which contain a much larger number of lines for this range of wave-length.

A classification by Duffield[1] may be used in comparing the displacements measured by him with the corresponding Zeeman separations. He forms three main groups according to amount of displace-

[1] *Op. cit.*, p. 160.

ment. Table VII gives the mean separation and displacement for each of these groups, at first singly, then combined so as to form two groups with more lines in each.

TABLE VII

MEANS OF SEPARATION AND DISPLACEMENT FOR DUFFIELD'S DISPLACEMENT GROUPS

Group Number	I (Unreversed)	I (Reversed)	II	III	I (Total)	II and III
No. lines	26	13	6	10	39	16
Mean sep	0.349	0.307	0.529	0.376	0.335	0.433
Mean displ	0.064	0.077	0.168	0.319	0.068	0.262
Classes sep. and displ	M:M	M:M	L:L	M:L	M:M	L:L

We see that separation and displacement are of the same order of magnitude except for Group III where some very large displacements correspond to medium separations. The material is better presented in the last two columns, where the larger number of lines give means of higher weight. These means show as before that a much larger range is covered by the displacements than by the separations.

Duffield has found that the displacement of the lines belonging to his three groups have very different rates of increase with increase of pressure, the lines of Group III showing the most rapid change. I have plotted Humphreys' measurements for lines taken at two and three pressures and find for them a great difference in steepness of the curves. There seems to be no general relation for individual lines between the rapidity of increase of displacement with pressure and their behavior as to Zeeman separation. Indeed, the fact that this difference exists for pressure effects is to some extent against a relation of the two phenomena, since a corresponding action in the Zeeman effect would mean a different rate of increase of displacement with field strength for different lines, which is contrary to experiment.

2. *Chromium.*—In the chromium spectrum 54 lines were available for a comparison of separation and displacement. The classes into which the values fall are tabulated in Table VIII in the same manner as in Table IV for iron.

Following the same method as with iron, we find 16 lines in Group 2 for which the magnitudes of separation and displacement are nearly

enough in the same class to come within possible errors of measurement, giving 52 per cent in fair agreement for this spectrum. The agreement of magnitudes for chromium is thus not so good as for iron. The few lines beyond λ 4700 show large values for the displacement, and have also as a rule large separations. A rough classification of magnitudes is given farther on.

TABLE VIII

SUMMARY OF CLASSES—CHROMIUM

Group	1			2				3	
Ratio of magnitude	S:S	M:M	L:L	S:M	M:S	M:L	L:M	S:L	L:S
Number of lines	2	5	5	9	9	2	10	1	11
Group total	12			30				12	
Group percentage	22			56				22	

3. *Titanium.*—A large amount of material is on hand for the Zeeman effect, but as the pressure measurements are scanty we have at present but 31 titanium lines in Table III for comparison. The magnitude groups for titanium are given in Table IX, corresponding to Tables IV and VIII.

TABLE IX

SUMMARY OF CLASSES—TITANIUM

Group	1			2				3		
Ratio of magnitude	S:S	M:M	L:L	S:M	M:S	M:L	L:M	S:L	L:S	O:M
Number of lines	6	2	6	1	5	4	5	0	1	1
Group total	14			15				2		
Group percentage	45			48				7		

The agreement for magnitude classes is much the same as for iron. Six lines of Group 2 may be within the error of measurement, giving 64 per cent in possible agreement as to magnitude. λ 4295.914 is a displaced line which shows no Zeeman separation.

ROUGH CLASSIFICATION FOR CHROMIUM AND TITANIUM

The small number of lines available for chromium and titanium does not justify an extended comparison of means such as is given

for iron in Tables V and VI; but a division according to large and small displacement, each of these classes including some lines regularly classed as medium, may be of some value. Table X is formed on this plan.

TABLE X

CHROMIUM AND TITANIUM LINES CLASSED ACCORDING TO DISPLACEMENT

	Chromium		Titanium	
Displacement range	<0.080	>0.080	<0.080	>0.080
No. lines	42	12	16	14
Mean separation	0.409	0.532	0.319	0.418
Mean displacement	0.061	0.127	0.049	0.113
Ratio of magnitudes	L:M	L:L	M:S	L:L

This table shows a fair agreement as to magnitudes, the mean separation corresponding to small displacement being somewhat large for both elements.

GENERAL REMARKS AND CONCLUSION

It might be expected that the very complex Zeeman separations which occur for many lines listed in Tables I, II, and III would not show as good an agreement with the pressure displacement as those of a simpler type. There appears, however, to be no general rule of this sort. Throughout the spectra many complex lines are found to agree in magnitude with the pressure displacement, while some of the largest discrepancies are found for triplets weighted 3, which is the simplest type of separation with components most sharply defined.

Until more complete pressure measurements are at hand, we cannot say how many lines may show large Zeeman separation and no perceptible displacement. While Humphreys' tables cover most of the strong lines for the region measured, there are some lines of considerable strength missing which are of large separation. It may be that blends and other disturbing features interfere in some cases on the pressure plates.

A much better comparison could be made if measurements for a large range of wave-length for both phenomena were available. It will be shown when the complete Zeeman table is published that the mean separation for all iron lines from λ 3700 to λ 6700 increases

closely with the square of the wave-length. The pressure displacements are found to have higher values in the green than in the blue and violet, but the number of measurements is not sufficient to give an accurate value for the rate of increase. This agreement of Zeeman and pressure effects in respect to increase with wave-length, at present qualitative, may furnish us the best evidence as to the real relation of the two phenomena. The matter presented in this paper, while it shows that for a majority of the lines considered there is a fair agreement as to magnitude of separation and displacement, shows also from the number and character of the lines not in agreement that this line of evidence, even if made very extensive, will scarcely be convincing as to pressure displacement being due to magnetic perturbations. The degree of concordance which we have could perhaps result entirely from the fact that the magnitude of each effect increases with the wave-length. This does not prove a close physical relation, since any theory of the pressure effect that might be offered would probably involve a change with the wave-length. The theory of Richardson requires an increase with the third power of the wave-length, that of Larmor a decrease with increasing wave-length. If, however, the investigation of both magnetic field and pressure effects can be made to cover all but the weakest lines of several many-lined spectra through a large range of wave-length, and if the rate of increase of mean pressure and mean displacement is found to be the same, we shall have established a high degree of probability that the causes of the two phenomena are fundamentally alike.

Mount Wilson Solar Observatory
April 25, 1910

Minor Contributions and Notes

NOTE ON THE PRESSURE-SHIFT OF VIOLET-SIDED SPECTRAL LINES

In that part of their excellent paper in which they discuss the comparison of the solar spectrum with that of iron in the air, Fabry and Buisson[1] say:

> Les raies à élargissement vers le violet ne se trouvent pas parmi celles que l'on a étudiées au point de vue de l'action de la pression; ces raies subissent, pour la variation de pression de 1 atmosphère lorsqu'on passe du vide à la pression atmosphérique, un déplacement apparent vers le violet; il serait très intéressant de savoir comment elles se comportent aux pressions élevées.

The first part of this statement applies to seven of the eight violet-sided lines listed by Fabry and Buisson, but not to the line λ 4250.78. The behavior of this line under pressure has been repeatedly examined, and its increase in wave-length found to be almost exactly 0.002 Ångström unit per atmosphere.[2]

Probably the best examples of the violet-sided unsymmetrical lines that have been examined under pressure are the sodium lines λ 3302.5 and λ 3303.1, both of which spread much more toward the violet than toward the red end of the spectrum. Under pressure, however, their wave-lengths increase approximately 0.007 Ångström unit per atmosphere.[3]

Unsymmetrical broadening, so marked in the case of certain lines with increase of material in the arc, and pressure-shift, appear to be due to different causes. There is, therefore, no a priori reason for thinking that unsymmetrical lines, whether red- or violet-sided, will give pressure-shifts very different from those of symmetrical lines. At any rate, such differences in the shifts of the several types as may appear to exist decrease as the measurements are confined to narrower reversals and finer lines.

[1] *Astrophysical Journal*, **31**, 111–12, 1910.

[2] Humphreys, *Ibid.*, **6**, 200, 1897; **22**, 218, 1905; **26**, 24, 1907; Duffield, *Phil. Trans.*, A, **208**, 136 and 138, 1908.

[3] *Astrophysical Journal*, **6**, 183 and 210, 1897.

Nevertheless, it is of distinct advantage to know which of the hundreds of lines most need to be examined in the laboratory; and it is now expected that the behavior under pressure of many additional lines will soon be determined—the selection being made in part to meet the requirements of recent papers by Evershed,[1] Adams,[2] Fabry and Buisson,[3] and others.

W. J. HUMPHREYS

MOUNT WEATHER OBSERVATORY, VA.

ADDITIONAL NOTES ON RADIAL VELOCITIES WITH OBJECTIVE-PRISM

Various salts of neodymium have been tried, but the chloride appears to be the best. The 4273 line in the nitrate has a broad, hazy extension in the side of longer wave-length, which makes it unsuitable for measurements. The sulphate shows the band as narrow as the chloride, but it is not nearly as soluble, and the cell must be 3 cm thick for a saturated solution.

I have photographed the 4273 line with the 21-foot concave grating, with an iron comparison spectrum. The center of the line can be determined easily to within 0.1 Ångström unit. Its width is a little less than 3 Å.U. The photograph is reproduced on Plate XII, Fig. 1. To determine whether any change or shift was produced by a change of temperature, two spectra were made with the 21-foot grating, one with the solution partly frozen, the other at a temperature of about 35° C. No difference in the position of the band could be detected, though it appeared to be a trifle narrower at the lower temperature, probably about 10° below the freezing point of water. Its wave-length referred to the iron lines 4271.30 and 4271.90 (Kayser and Runge) is 4272.90.

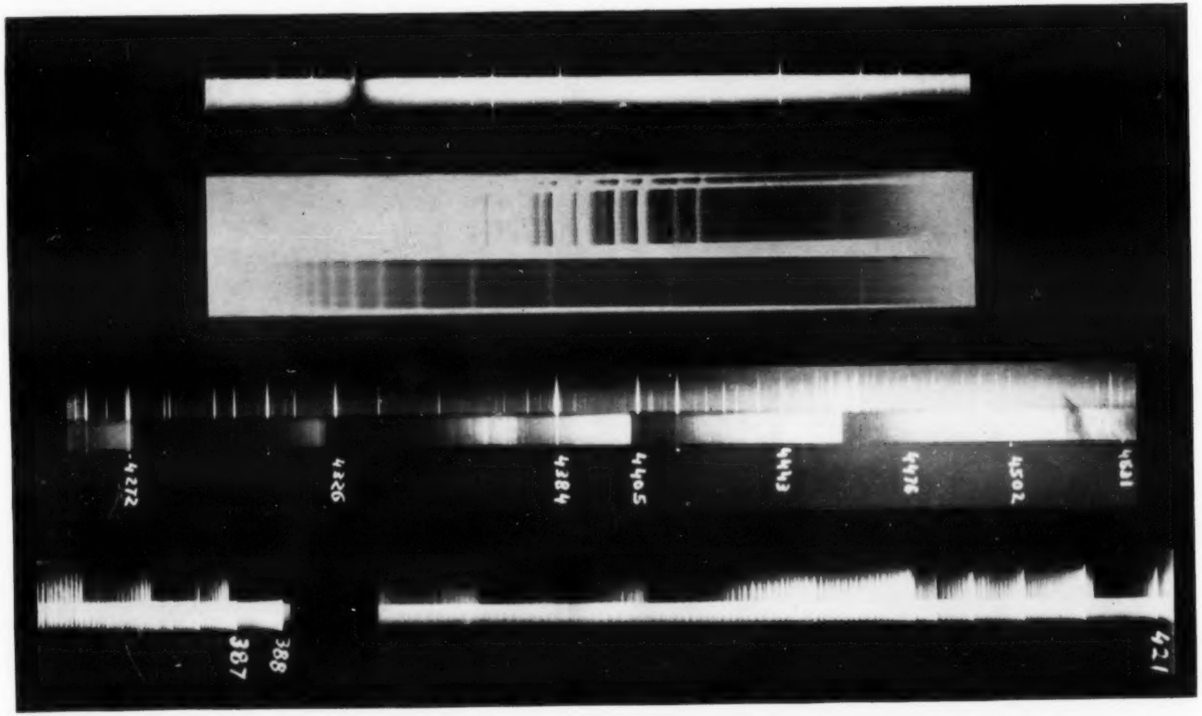
Through the courtesy of Professor E. C. Pickering, the facilities of the Harvard Observatory were placed at my disposal, and. aided by Mr. King, I made some experiments with other absorbing media, which previous examination had shown to be promising.

[1] *Memoirs Kodaikanal Observatory*, **1**, 1, 1909.

[2] *Astrophysical Journal*, **31**, 30, 1910.

[3] *Ibid.*, **31**, 97, 1910.

PLATE XII



1. Neodymium chloride with Fe^- comparison.
2. α *Leonis* with and without filter of peroxide of chlorine.
3. Peroxide of chlorine with iron comparison.
4. Peroxide of chlorine with carbon arc.

The first substance tried was manganese perchloride, a yellowish gas, prepared by adding a few fragments of fused chloride of sodium to a solution of permanganate of potash in concentrated sulphuric acid, and warming gently. Care should be used in preparing this gas, as the reaction is sometimes explosive. It shows some narrow bands in the green and greenish yellow, when used with an isochromatic plate. They are not much narrower than the λ 4273 line of neodymium, as subsequently observed with the 21-foot grating, though they appeared much narrower in the star photographs, owing to the smaller dispersion of the prism in this region. I doubt if much is to be gained by the use of this substance.

Peroxide of chlorine was next tried, but it attacked the sealing-wax with which the glass plates of the cell were cemented, rapidly disappearing, and we obtained only one or two rather unsatisfactory photographs.

This substance I have since examined with the 21-foot grating, and find it to be admirably adapted to the purpose. The bands are as sharp on one edge as the iron lines, but shade off toward the ultra-violet. The position of the edge of the shaded bands does not vary with the density of the gas, and their positions can be determined to within 0.02 Å.U. on the photographs made with the grating. Unfortunately the absorption bands cover most of the hydrogen lines, so that it cannot be used very well for stars of the first type. It is possible, however, that the lines λ 4863 and λ 3837 will appear. For stars of other types the peroxide of chlorine seems to be all that can be desired. It is easily prepared by adding a few crystals of chlorate of potash to a little concentrated sulphuric acid contained in a test tube, and warming. The cell should be made of a glass ring about 1 cm thick and two circular glass plates, fastened together with some non-organic cement. Probably a mixture of water glass and powdered asbestos will answer the purpose, though some cement which never becomes quite hard would be preferable, as the cell could then be easily taken apart and cleaned.

Photographs made with the 21-foot grating are reproduced on Plate XII, Figs. 3 and 4.

The heads of the bands have been determined to within 0.02 Å.U. They are as follows: $\lambda\lambda$ 4273.20, 4324.38, 4402.93, 4458.15, 4199.42,

4062.23, 3880 (only roughly determined as yet). I see no reason why velocities cannot be determined to within 2 or 3 kilometers by means of these bands, provided of course that the rest of the apparatus is brought to such a degree of perfection as to enable them to be used to their full advantage.

To do this, with long exposures, it will of course be necessary to keep the prism at a constant temperature; atmospheric disturbances will probably put the final limit to the accuracy of the determinations.

A photograph of *α Leonis* made by Mr. King with and without the screen of peroxide of chlorine is reproduced in Fig. 2. The gas was not sufficiently diluted, however, and the extreme violet and ultra-violet were absorbed.

R. W. Wood

Johns Hopkins University
April 22, 1910

SIR WILLIAM HUGGINS

It is with the deepest regret that we record the death, on May 13, of our eminent collaborator Sir William Huggins, whose pioneer discoveries in astrophysics have made his name immortal in the annals of science.

Sir William had on February 7 completed his eighty-sixth year, in the fullest possession of his remarkable mental faculties, and with the liveliest interest in every step of progress in the branch of science which he had so largely helped to found.

His skill as an observer was equaled by his philosophical judgment in the interpretation of the results of observations, and his simple dignity was the crowning evidence of his greatness.

In offering our sincerest sympathy to his bereaved consort and coadjutor, we can remotely sense the magnitude of her loss, after many years of the closest association; for we measure it in terms of the real attachment to him resulting from his generous friendship to younger men, even when personal meetings were infrequent and communication was chiefly by letters.

A photogravure from Sir John Collier's fine portrait of Sir William Huggins was published in this *Journal* in September 1907 (Vol. **26,** facing p. 128). An appropriate account of his life and work will be published in a subsequent number of this *Journal.*

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Articles written in any language will be accepted for publication, but unless a wish to the contrary is expressed by the author, they will be translated into English. Tables of wave-lengths will be printed with the short wave-lengths at the top, and maps of spectra with the red end on the right unless the author requests that the reverse procedure be followed.

Accuracy in the proof is gained by having manuscripts type-written, provided the author carefully examines the sheets and eliminates any errors introduced by the stenographer. It is suggested that the author should retain a carbon or tissue copy of the manuscript, as it is generally necessary to keep the original manuscript at the editorial office until the article is printed.

All drawings should be carefully made with India ink on stiff paper, usually each on a separate sheet, on about double the scale of the engraving desired. Lettering of diagrams will be done in type around the margins of the cut where feasible. Otherwise printed letters should be put in lightly with pencil, to be later impressed with type at the editorial office, or should be pasted on the drawing where required.

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